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ON THE THEORY OF RELATIVITY: PHILOSOPHICAL ASPECTS.

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I. Introduction.

THOSE who look on physics from the outside not infrequently have the feeling that it has forgotten some of its philosophical foundations. And among physicists themselves this condition of their science has not entirely escaped notice.[1]

The physicist who, above all other men, has to deal with space and time has fallen into conventions concerning them of which he is often not aware. It may be true that these conventions are exactly the ones which he should make. It is certain, however, that they should be made only by one who is fully conscious of their nature as conventions and not as absolute realities beyond the power of the investigator to modify.

Likewise, a question arises as to what element of convention is involved in our usual conceptions of mass, energy, etc.; that the question is not easily answered will become apparent on reflection.

These and many other considerations suggest the desirability of a fresh analysis of the foundations of physical science. Now it is a ground of gratulation for all those interested in this matter that there has arisen within modern physics itself a new movement capable of contributing most effectively to the construction of a more satisfactory foundation for its superstructure of theory. I refer to the recent widespread interest in the principle of relativity and the rapid developments to which it has led.

It is at once admitted that the theory of relativity is not yet established on an experimental basis which is satisfactory to all persons; in fact, some of those who dispute its claim to acceptance are among the most

[1] Compare the discussion given by Crew, Physical Review, 31, 79–92.

eminent men of science of the present time. On the other hand there is a large and growing body of workers who are rapidly pushing forward investigations the inspiration for which is afforded by the theory of relativity.

This state of affairs will probably give rise to a considerable controversial literature. If the outcome of this controversy is the acceptance in the main of the theory of relativity, then this theory will afford just the means needed to arouse in investigators in the field of physics a lively sense of the philosophical foundations of their science. If the conclusions of relativity are refuted this will probably be done by a careful study of the foundations of physical science and a penetrating analysis of the grounds of our confidence in the conclusions which it reaches. This of itself will be sufficient to correct the present tendency to forget the philosophical basis of the science.

It follows that in any event the theory of relativity is certain to force a fresh study of the foundations of physical theory. If it accomplishes no more than this it will have done well.

The object of this paper is to point out some of the more immediate and fundamental conclusions of a philosophical nature to which the theory of relativity gives rise, especially those related to the foundations of physics. An attempt is made to present the discussion in such form as to be intelligible to one who is unacquainted with the theory of relativity; this is done for the convenience of those who have not hitherto been interested in the matter. It will probably be found desirable, however, to have as much previous knowledge of the subject as may be obtained from Comstock's paper in *Science*, N. S., 31 (1910), 767–772.[1]

II. The Fundamental Basis of Relativity.

The fundamental basis of the theory of relativity may be set forth in a few statements each of which is a generalization from experiment. In so far as these statements are generalizations they are of course unproved experimentally. But they are nevertheless supposed to be the natural teachings of experiment; and as such are to be accepted with a good degree of confidence unless it can be shown that they are in contradiction to other known experimental facts.

The first of these statements gives expression to a law which is suggested by the famous Michelson-Morley experiment,[2] the object of which was to try out a certain theory concerning the ether by ascertaining

[1] One who desires to go further into the subject might consult with profit the author's previous papers in the PHYSICAL REVIEW, Vol. 35, Series 1, pp. 153–176; Vol. I., Series 2, pp. 161–178.

[2] See American Journal of Science (3), 34 (1887), 333–345.

whether a result predicted by it would be found by actual test. For a long time it had been supposed that the known facts about light, electricity and magnetism required for their explanation the theory that the ether of space is stationary. Such a conclusion led to the belief that the velocity of the earth through the ether could be determined by optical experiments. Thus it was predicted that the time which would be required for a beam of light to pass a given distance and return would be different in the two cases when the path of light was parallel to the direction of the motion and when it was perpendicular to this direction. The object of the Michelson-Morley experiment, as we have said, was to put this prediction to a crucial test.

The experiment was a bold one, seeing that the velocities to be measured were so little different; and yet it was carried out in such a brilliant way as to permit no serious doubt of the accuracy of the results. The difference of velocity predicted by theory was found by experiment not to exist; there was not the slightest difference of time in the passage of light along two paths of equal length, one in a direction parallel to the earth's motion and the other in a direction perpendicular to it.

There are different points of view from which one may look at this experiment. In the theory of relativity it is taken in the light of an attempt to detect the earth's motion through space by means of the effect of this motion on terrestrial phenomena. So far as the experiment goes, it indicates that such motion cannot be detected in this way. Furthermore, no one has yet been able to devise an experiment by means of which the earth's motion through space can be detected by observations made on the earth alone. The question arises: Is it possible to have any such experiment at all? In the theory of relativity this question is answered in the negative. The Michelson-Morley experiment and other experiments are thus generalized into one of the fundamental laws or postulates of relativity, which may be stated as follows:

A. The uniform translatory motion of any system cannot be detected by an observer traveling with the system and making observations on it alone.

Another part of the fundamental basis of the theory of relativity is a principle which has long been familiar in the theory of light and has never been found in disagreement with experimental facts. It may be stated thus:

B. The velocity of light in free space is independent of the velocity of the source of light.

By means of laws *A* and *B*, taken in connection with certain principles universally accepted in the classical mechanics, it may be shown[1] that

[1] See a treatment by the author in the first paper referred to above.

the velocity of light is independent of the direction of motion of the observer. Thus we have:

C. THEOREM. *The velocity of light in free space as measured by any observer is independent of the direction of motion of that observer.*

We are thus led to inquire further as to whether the velocity of light in free space is independent of the absolute value of the velocity of the observer. It appears to be impossible to prove that it is so independent; and yet one is unable to conceive of any way in which it could be dependent on the absolute value without at the same time depending on the direction of the velocity also. But, accepting *A* and *B*, we prove *C* which asserts that such dependence does not exist. There seems, then, nothing left to do but to *assume* that dependence on absolute value does not exist; and this is what is done in the theory of relativity. In the absence of experimental information in contradiction to this assumption it is undoubtedly the natural one to make. Any other procedure would be out of harmony with the usual methods of science. This assumption, therefore, is one which we make as the most natural teaching of experimental facts; and as such we treat it as a "law of nature" so long as fresh experiment does not invalidate it. This law may be stated as follows:

D. The velocity of light in free space as measured by any observer is independent of the absolute value of the velocity of the observer.

The law which we have stated as *A* above is often referred to as the first postulate of relativity. *B*, *C* and *D* taken together constitute the second postulate of relativity. We have broken this postulate into parts in order to state clearly just how it depends on experiment. Combining the parts we may state its essential content as follows:

E. The velocity of light is independent of the relative velocity of the source of light and the observer.

The theory of relativity consists of those conclusions which can be derived by logical process (that is, mathematically) from *A* and *E* in conjunction with certain principles which are universally accepted in the classical mechanics—at least, this may be taken as a tentative definition of the theory of relativity. If one agrees that experiment has been properly generalized in *A*, *B* and *D* one has therefore the alternative of accepting the conclusions of relativity or of giving up almost the whole of the usual system of mechanics. That one should take the first horn of the dilemma hardly admits question.

III. FUNDAMENTAL CONCLUSIONS OF THE THEORY OF RELATIVITY.

For the sake of convenience in stating some of the fundamental conclusions of the theory of relativity let us suppose that we have two obser-

vers A and B placed on platforms moving with respect to each other. Let us suppose that A is on a platform denoted by S_1 and that B is on a platform denoted by S_2. Suppose that to A the platform S_2 appears to move with the velocity v in the direction indicated by the arrow at S_2; then to B the platform S_1 will appear to move with velocity v in the direction indicated by the arrow at S_1. Suppose further that the units of length employed by A and B are such that they arrive at the same numerical results in measuring the length MN, MN being perpendicular to the line of relative motion of S_1 and S_2. The platform S_1 and the instruments employed by A for measuring time and length will be spoken of as the system of reference S_1. Similarly, we shall speak of the system of reference S_2. For convenience we shall use β to denote the ratio v/c, where c is the velocity of light.

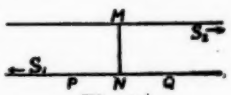

Fig. 1.

The two systems of reference S_1 and S_2 being thus defined, the question arises as to how the units of length and of time of S_1 are related to the corresponding units of S_2. If we accept as true the laws stated in A and E of the preceding section—as, in fact, is done in the theory of relativity—we are led to some very remarkable conclusions. We state first the relation of the time units:

To an observer A on S_1 the time unit of S_1 appears to be in the ratio $\sqrt{1-\beta^2} : 1$ to that of S_2, while to an observer B on S_2 the time unit of S_2 appears to be in the ratio $\sqrt{1-\beta^2} : 1$ to that of S_1.

Thus if A compares his clocks with those of B it will appear to A that A's clocks are running faster than B's clocks; on the other hand, if B compares his clocks with those of A it will appear to B that B's clocks are running faster than A's clocks. Thus the two observers are in hopeless disagreement as to the measurement of time. An analysis of this divergence is made below in section V.

Another matter of fundamental importance in the measurement of time will be brought to attention by the question: When are two events which happen at different places to be considered simultaneous? The nature of the difficulty can be seen from the following result in the theory of relativity, an analysis of which is to be found below in section VI.:

If an observer A on S_1 places two clocks at a distance d apart in a line parallel to the line of relative motion of S_1 and S_2 (say at P and Q respectively) and adjusts them so that they appear to him to mark the same time: then to an observer B on S_2 the clock on S_1 which is forward in point of motion appears to be behind in point of time by the amount

$$\frac{v}{c^2}\frac{d}{\sqrt{1-\beta^2}}.$$

Let us consider the units of length in S_1 and S_2. If A and B make measurements of length in the direction MN their results are in perfect agreement in the theory of relativity as in the classical mechanics; but the state of matters is very different when measurement is made in a line parallel to the line of relative motion of S_1 and S_2, as one sees from the following theorem:

Let l denote a line parallel to the line of relative motion of S_1 and S_2. Then to an observer A on S_1 the unit of length of S_1 along l appears to be in the ratio $\sqrt{1-\beta^2} : 1$ to that of S_2 while to an observer on S_2 the unit of length of S_2 along l appears to be in the ratio $\sqrt{1-\beta^2} : 1$ to that of S_1.

Thus it appears that when A and B are measuring length in a line parallel to their line of relative motion they are in hopeless disagreement. What this result signifies we shall attempt to explain in section IV.

From the results stated above it may be shown that:

The velocity of light is a maximum which the velocity of a material body may approach but can never reach.

If one brings into consideration the mass of a moving body another result, essentially equivalent to that just given, may also be obtained; this may be stated as follows (see further discussion in section VIII. below):

No finite force is sufficient to give a material particle a velocity as great as that of light.

In classical mechanics it is customary to assume that the mass of a given body is constant and that it is independent of the direction in which the mass is measured. But if the principle of relativity is accepted it follows that neither of these conclusions is valid. It turns out that the mass of a body depends on its velocity and also on the direction, relative to the line of motion of the body, along which that mass is measured. For convenience in distinguishing these measurements of mass we shall speak of the "transverse mass" as that with which we must reckon when we consider motion in a line perpendicular to the line of relative motion of the systems S_1 and S_2; when the motion is parallel to this line we shall speak of the "longitudinal mass" of the body.

The general conclusions of the theory of relativity concerning mass may now be stated as follows:

Let m_0 denote the mass of a body when at rest relative to a system of reference S. When it is moving with a velocity v relative to S, denote by t_v its transverse mass, that is, its mass in a line perpendicular to its line of motion. Similarly, denote by l_v its longitudinal mass. Then we have

$$t_v = \frac{m_0}{(1 - \beta^2)^{\frac{1}{2}}},$$

$$l_v = \frac{m_0}{(1 - \beta^2)^{\frac{1}{2}}}.$$

IV. THE NOTION OF LENGTH.

In the preceding section we saw that two observers A and B on relatively moving systems of reference S_1 and S_2 respectively are in disagreement as to units of length along a line l parallel to their line of relative motion. This disagreement is of a very peculiar character. To A it appears that B's units are longer than his own. On the other hand, it seems to B that his units are shorter than A's. In the two cases the apparent ratio is the same; more precisely, the unit which appears to either observer to be the shorter seems to him to have the ratio $\sqrt{1 - \beta^2} : 1$ to that which appears to him to be the longer. Although they are thus in disagreement there is yet a certain symmetry in the way in which their opinions diverge.

Let us suppose that these two observers now undertake to bring themselves into a closer agreement in measurements of length along the line l. Suppose that B agrees arbitrarily to shorten his unit so that it will appear to A that the units of A and B are of the same length. Then, so far as A is concerned, all difficulty has disappeared. How is B affected by this change? We see that the difficulty which he experienced is not disposed of; on the other hand it is greater than before. Already, it seemed to him that his unit was shorter than A's. Now, since he has shortened his unit, the divergence appears to him to be increased. Moreover, the symmetry which we found in the former case is now absent.

Furthermore, if any other changes in the units of A and B are made we shall always find difficulties as great as or greater than those which we encountered in the initial case. There is no other conclusion than this: We are face to face with an essential difficulty—one that is not to be removed by any mere artifice. What account of it shall we render to ourselves?

This much is already obvious: The length of an object is not an absolute something; it depends upon the measurer in an essential way.

We have just spoken of the length of an object, *a material object*. One can hardly refrain from raising the question of the *abstract* notion of length as apart from any material thing having length. But this problem has new difficulties of its own. All lengths of which I have experience are lengths of material objects or lengths between material

objects. Can one formulate in the mind the notion of length as apart from these material things which are measured; and, if so, how can such a notion of length be applied experimentally to the measurement of material things?

Questions of this nature we shall lay aside and shall return to the consideration of the length of material objects.

We have certain intuitive notions concerning the nature of matter which it is necessary for us to examine now. We have usually supposed that to revolve a steel bar, for instance, through an angle of ninety degrees has no effect upon its length. Let us suppose for the moment that this is not so; but that the bar is shorter when pointing in some directions than in others, so that its length is the product of two factors one of which is its length in a certain initial position and the other of which is a function of the direction in which the body points relative to that in the initial position. Suppose that at the same time all other objects experience precisely the same change for varying directions. It is obvious that in this case we should have no means of ascertaining this dependence of length upon the direction in which the body points.

To an observer placed in a situation like this it would be natural to assume that the length of the steel bar is the same in all directions. In other words, in arriving at his definition of length he would make certain conventions to suit his convenience.

Now suppose that the system of such an observer is set in motion with a uniform velocity v relative to the previous state of the system; and that at the same time all bodies on his system undergo simultaneously a continuous dilatation or contraction. This observer would have no means of ascertaining that fact; and accordingly he would suppose that his steel bar had the same length as before. In other words, he would unconsciously introduce a new convention concerning his measurement of length.

There is no *a priori* reason why our actual universe should not be such as the hypothetical one just described. To suppose it so unless our experience demands such a supposition would be unnatural; because it would introduce an unnecessary inconvenience. But suppose that in our growing knowledge of the universe there should come a time when we could more conveniently represent to ourselves the actual facts of our experience by supposing that all material things are subject to some such deformations as those which we have indicated above; there is certainly no *a priori* reason why we should not conclude that such is the essential nature of the structure of the universe.

Naturally we would not come to this conclusion without due considera-

tion. We would first enquire earnestly if there is not some more convenient way by which we can reconcile all experimental facts; and only in the event of a failure to find such a way would we be willing to so profoundly modify our views of the material world.

Now, if we agree to suppose that our actual universe is subject to a certain (appropriately defined) deformation of the general type discussed above it would follow that observers A and B on the respective systems S_1 and S_2 would be in just such disagreement as to units of length as that which exists, according to the theory of relativity. Therefore, that which at the outset seemed to be of such essential difficulty is easily enough explained, *if we are willing to modify so profoundly our conception of the nature of material bodies.*

Whether in the present state of science experimental facts demand such a radical procedure is a question which will be answered differently by different minds. To one who accepts the postulates of relativity there is indeed no other recourse; one who refuses to accept them must find some other satisfactory way to account for experimental facts. The Lorentz theory of electrons gives striking evidence in favor of supposing that matter is subject to some such deformations as those mentioned above; and this evidence is the more important and interesting in that the deformations (as conceived in this theory) were assumed to exist simply in order to be able to account directly for experimental facts.

V. The Measurement of Time.[1]

That two observers in relative motion are in hopeless disagreement as to the measurement of length in their line of relative motion is a conclusion which is probably (at first) sufficiently disconcerting to most of us; and some no doubt have the feeling that any explanation of it so far offered is at best artificial and does not reach the root of the matter. But it is an even greater shock to intuition to conclude, as we are forced to do according to the theory of relativity, that there is a like ineradicable disagreement in the measurement of time. A discussion similar to that in the preceding section brings out the fact that our observers A and B cannot possibly arrive at consistent means of measuring intervals of time. The treatment is so far similar to the preceding discussion for length that we need not repeat it; we shall content ourselves with a brief discussion of conclusions to be drawn from the matter.

Why is this inability of A and B to agree in measuring time received

[1] In connection with this section and the following one the reader should compare the excellent and interesting treatment of the problem of measuring time to be found in Chapter II. of Poincaré's Value of Science (translated into English by Halsted).

in our minds with such a distinct feeling of surprise and shock? It is doubtless because we have such a lively sense of the passage of time. It seems to be a thing which we know directly, and the conclusion in question is contrary to our unsophisticated intuition concerning the nature of time.

But what is it that we know directly? We have an immediate perception of what it is for two conscious phenomena to coexist in our mind; and consequently we perceive immediately the simultaneity of events in our minds. Further, we have a perfectly clear sense of the order of succession of events in our own consciousness. Is that not all that we know directly?

The difficulties which A and B experience in correlating their measurements of time grow out of two things, of neither of which we have direct perception.

In the first place there are two consciousnesses involved; and what reason have we to suppose that succession of events is the same for these two? This question we shall not treat, assuming that the principal matter can be put into such impersonal form as to obviate this difficulty altogether. (As a matter of fact, so far as anything characteristic of the theory of relativity is concerned this can be done.)

The other difficulty has to do with the measurement of time as opposed to the mere psychological experience of its passage. In this matter we are absolutely without any direct intuition to guide us. We have no immediate sense of the equality of two intervals of time. Therefore, whatever definition we employ for such equality will necessarily have in it an important element of convention. To keep this well in mind will facilitate our discussion.

Our problem is this: How shall we assign a numerical measure of length to a given time-interval; say to an interval in which a given physical phenomenon takes place? We shall arrive at the answer by asking another question: Why should we seek to measure time-intervals at all, seeing that we have no immediate consciousness of the equality of such intervals? There can be only one answer: we seek to measure time as a matter of convenience to us in representing to ourselves our experiences and the phenomena of which we are witnesses. In such a way we can render to ourselves a better account of the world in which we live and of our relation to it.

Now, since our only reason for attempting to measure time is in a matter of convenience, the way in which we measure it will be determined by the dictates of that convenience. The system of time measurement which we shall adopt is just that system by means of which the laws of nature may be stated in the simplest form for our comprehension.

Let us return to the case of the two observers A and B of section III. Suppose that each of them has chosen a system of measuring time that suits his convenience in the interpretation of the laws of nature on his system. There is no *a priori* reason why the two observers should measure time-intervals in the same way. In fact, since there is an arbitrary element in the case of each method of measurement and since the two systems are in a state of relative motion, it is not at all unnatural that the units of A and B should differ.

Now it is to be noticed that each of the observers A and B is in just the situation in which we find ourselves. We have chosen a method of measuring time which seems to us convenient. In so far as that method depends on convenience it is relative to us who are observers, and therefore it has in it something which is arbitrary. There is no doubt that it would be desirable for us to know what it is which is arbitrary, which is relative to us who observe; but it is equally obvious that it must be difficult for us to determine what this arbitrary element is.

The theory of relativity makes a contribution to the solution of this problem. We suppose that two observers on different systems find the laws of nature the same as we find them; or, more exactly, we suppose that they find certain specific laws the same as we find them. Then we inquire as to their agreement in measuring time and see that they differ in a certain definite way. This difference is due to things which are relative to the two observers; and thus we begin to get some insight into the ultimate basis of our own method of measurement.

The matter will become clearer if we speak of the simultaneity of events which happen at different places; and therefore we turn to a discussion of this topic.

VI. Simultaneity of Events Happening at Different Places.

What shall we mean by saying that two events which happen at different places are simultaneous?

First of all it should be noticed that we have no direct sense of what this statement should mean. I have a direct perception of the simultaneity of two events in my own consciousness. I consider them simultaneous because they are so interlocked that I cannot separate them without mutilating them. If two things happen which are far removed from each other I do not have a direct perception of both of them in such way that I perceive them as simultaneous. When should I consider such events to be simultaneous?

To answer this question we are forced to the same considerations as those which we met in the preceding section. There can be no absolute

criterion by which we shall be able to fix upon a definition as the only appropriate one. We must be guided by the demands of convenience, and by this alone.

In view of these considerations there is nothing unthinkable about the conclusion (in the theory of relativity) concerning simultaneity which we have given in section III. An observer A on one system of reference regulates clocks so that they appear to him to be simultaneous. It is apparent that to him the notion of simultaneity appears to be entirely independent of position in space. His clocks, even though they are separated by space, appear to him to be running together, that is, to be together in a sense which is entirely independent of all considerations of space.

But when B from another system of reference observes the clocks of A's system they do not appear to him to be marking simultaneously the same hour; and their lack of agreement is proportional to their distance apart, the factor of proportionality being a function of the relative velocity of the two systems.

Thus instants of time at different places which appear to A to be simultaneous in a sense which is entirely independent of all considerations of space appear to B in a very different light; namely, as if they were different instants of time, one preceding the other by an amount directly proportional to the distance between the points in space at which events occur which mark these instants. Even the order of succession of events is in certain cases different for the two observers, as one can readily verify.

It thus appears that the notion of simultaneity is relative to the system on which it is determined. In other words, *there is no such thing as the absolute simultaneity of events which happen at different places.* The only meaning which simultaneity can have is that which is given to it by convention.

Remark.—The difficulties which we have investigated in this and the preceding section are not peculiar to the theory of relativity. The fact that the quantitative measurement of time is relative to the observer who measures it has been insisted upon by philosophers, notably by Poincaré in his Value of Science already referred to. The considerations on which these conclusions have been based have been largely of a speculative character.

The interest of the subject from the point of view of the theory of relativity is that here we have an experimental basis for the conclusions which were previously reached by speculative considerations. Starting out from certain laws which we have (tentatively) accepted as demonstrated by experiment we have by logical processes alone reached these

remarkable conclusions concerning the nature of time as conceived by us. Thus we have experimental demonstration of important results previously reached only by speculative considerations.

In the very nature of things speculation must often outrun experiment. The philosopher vaguely and boldly conceives a truth which it requires years of patient labor to establish on a firm and satisfactory foundation.

It is one of the glories of modern science that things hitherto of a speculative nature are being brought under the domain of experimental fact. In the process many speculations are overturned and thrown to the winds; but that which is really of value we may believe will always be safe and will some day find its justification in results achieved in the laboratory.

VII. Time as a Fourth Dimension.

I have no intention of asserting that time is a fourth dimension of space in the sense in which we ordinarily employ the word "dimension"; such a statement would have no meaning. I wish to point out rather that it is in some measure connected with space, and that in many formulæ it must enter as it would if it were essentially and only a fourth dimension.

This will come out clearly if we consider the Einstein formulæ of transformation from one system of reference to another. These have been worked out in detail in several different places,[1] and the result will be assumed here. It may be stated as follows:

Let us consider three mutually perpendicular axes Ox, Oy, Oz of which Ox is in the line of relative motion of two systems of reference S and S'. Likewise let $O'x'$, $O'y'$, $O'z'$ be three mutually perpendicular axes parallel respectively to Ox, Oy, Oz. Let the first be fixed to S and the second to S'. At time $t = 0$ let O and O' coincide. Then if t, x, y, z; t', x', y', z' are the time and space coördinates on S and S' respectively, we have

$$t' = \frac{1}{\sqrt{1 - \beta^2}}\left(t - \frac{v}{c^2}x\right),$$

$$x' = \frac{1}{\sqrt{1 - \beta^2}}(x - vt),$$

$$y' = y,$$

$$z' = z,$$

where c is the velocity of light, v the relative velocity of S and S', and $\beta = v/c$.

In these formulæ the time variable t enters in a way precisely analogous to that in which the space variables x, y, z enter.

[1] Compare Physical Review, Vol. 35, pp. 171–173.

Suppose now that the law of some phenomenon as observed on S' is given by the equation

$$F(x', y', z', t') = 0$$

and we desire to know the expression of this law on S. We substitute for x', y', z', t', their values in terms of x, y, z, t given above; and thus obtain an equation stating the law in question.

From these considerations it appears that in many of our problems, namely, in those which have to do at once with two or more systems of reference, the time and space variables taken together play the rôle of four variables each having to do with one dimension of a four-dimensional continuum.

This conclusion alone raises philosophical questions of profound importance concerning the nature of space and time; but into these we cannot enter here.

VIII. A Maximum Velocity for Material Bodies.

There are several ways by which it may be shown that a material body cannot have a velocity as great as that of light. One of the simplest is that which comes from a consideration of mass. Let us consider the equation

$$l_v = \frac{m_0}{(1 - \beta^2)^{\frac{1}{2}}},$$

where m_0 is the mass of a body at rest relative to a given system of reference S, l_v is the longitudinal mass of the body moving with a velocity v with respect to S. If we consider larger and larger values of the velocity v we see that l_v increases and becomes infinite as v approaches c. This is equivalent to saying that the longitudinal mass of any material body becomes infinite as the velocity of that body approaches c. Therefore it would require an infinite force to give to a material body the velocity c; that is, c is a maximum velocity which the velocity of a material body may approach but can never reach.

We may obtain the same result in a different way, and thus arrive at a deeper understanding of the matter. From the first formula of transformation given in the preceding section we have

$$t' \sqrt{1 - \beta^2} = t - \frac{v}{c^2} x.$$

Now suppose that $v/c > 1$ and that t and x are real. Then t' is imaginary. Hence, if two systems have a relative velocity greater than that of light, the time measurement in one of them is expressed as an imaginary

number; which is clearly absurd. Hence the relative velocity of the two systems is less than c, since $v = c$ obviously leads to an absurdity.

This conclusion concerning the maximum velocity of a material body brings up important considerations concerning the essential nature of mass and material things. How shall we conceive of matter so that it should have this astonishing property?

In the present state of science any answer to this question must necessarily be of a speculative nature; but it is probably worth while to mention briefly a theory of mass which is consistent with the existence of a maximum velocity for a material body.

Let us suppose that the mass of a piece of matter is due to a kind of strain in the ether, and that this strain is principally localized in a relatively small portion of space, but that from this center of localization there go out to infinity in all directions lines of strain which belong essentially to the piece of matter. (We make no assumption as to how this strain is set up; it may be due largely or entirely to the motion of electrons in the molecules of the matter.) Suppose that these lines of strain, except in the immediate neighborhood of the center of localization, are of such nature as to escape detection by our usual methods. Suppose further that when the piece of matter is moved, that is, when the center of localization is displaced, these lines of strain have a corresponding displacement, but that the ether of space resists this displacement, the degree of resistance depending on the velocity.

If the mass of matter is due to such a strain in the ether it is natural to suppose that mass is a measure of the amount of that strain. But, on our present hypothesis, we see that when matter is moved through space there is an increase of the strain on the ether due to such motion. This manifests itself to us in the way of an increase in the mass of the given piece of matter.

Moreover, when the body is in motion it is natural to suppose that these lines of strain are not distributed evenly in all directions. On account of this fact it would not be a matter for surprise if the mass of a moving body were different in different directions.

It thus appears that appropriate hypotheses (which have nothing in them inherently unnatural) would lead us to expect the same descriptive properties of mass as those which are actually found to exist if one accepts the postulates of relativity. Hence we conclude that there is nothing *a priori* improbable in the conclusions of relativity concerning the nature of mass. Therefore if we find satisfactory grounds for accepting the initial postulates of relativity, we shall not throw them overboard because of the strange conclusions concerning mass to which they have led us.

IX. ABSOLUTE REST. ABSOLUTE POSITION IN SPACE.

To any one who has examined critically the foundations of the theory of relativity it has certainly become apparent that the unexpected conclusions to which one is led concerning the relations of systems of reference is intimately connected with the point of view which the observer on each system takes in making his measurements. Each observer assumes his system to be at rest; and there seems to be no reason for preferring one assumption to the other or for replacing both of them by a third.

If, however, there were such a thing as absolute rest, or absolute position in space, the matter would be different. Each observer should reckon his velocity relative to that absolute; and the reason for divergence which existed before would no longer be found. We should, however, find it necessary to revise many of our "laws of nature," among them the postulates of relativity, if these laws are to be stated with reference to an absolute.

Since it appears to the present writer that absolute rest and absolute position are indefinable, this matter will be dismissed without further consideration.

X. RELATION OF THE THEORY OF RELATIVITY TO THE PHILOSOPHICAL CONTROVERSY CONCERNING THE ONE AND THE MANY.

The author is probably due the reader an apology for injecting into this paper any remarks concerning the abstruse and difficult question as to whether the universe is monistic or pluralistic—a question which has engaged philosophers from the time of the earliest Greek thinkers down to our own day. But if any one of the special sciences has some light to throw on a question of such profound general importance it seems desirable that those engaged in the study of that science should make it known.

If we accept the statement of one of the most eminent philosophers of our day that the universe is one to us in so far (and only in so far) as we know the connections by which it is bound into a One, it will doubtless be interesting to us to examine in what way the theory of relativity leads us to observe new connections. To speak briefly of these is my purpose in the present section.

1. We have already seen that time is not something apart from one who measures it and the system to which he belongs, and that (so far as we are concerned, at least) there is an intimate interlocking of time and space in an essential manner so that we are not able absolutely to extricate the one from the other. That is to say, there is a mutual interdependence of space and time for us who measure them so that we have

to consider them not as mutually exclusive "forms" of thought but as parts of a larger matrix, a sort of four-dimensional continuum, which at once includes both of them and binds them into a unity higher than that of either of them.

2. The theorem which we stated in section III. concerning units of length in the line of relative motion of two systems of reference not at rest relative to each other leads to the conclusion that when a material body is set in motion it undergoes a shortening in the direction of the line of motion and that this deformation takes place as it were automatically without the expenditure of work. This brings to light a very peculiar connection between material bodies and the "empty space" in which they lie or through which they move. Compare the discussion of mass in section VIII.

3. In the classical mechanics the mass of a body is an invariable quantity. In particular, it is independent of the velocity with which the body moves. Its measure is the same in whatever direction it is measured. In section III. we have already seen that none of these statements are true in the theory of relativity. The mass of a body is intimately connected with the absolute value of the velocity with which it moves relative to a system of reference and also (what is stranger still) with the direction in which the mass is measured relative to the direction of motion of the body.

4. It may also be shown[1] that the measure of gravitation depends in a similar way on the motion of the system on which it is measured.

5. Again, the total amount of energy[2] which a body possesses is a simple linear function of its transverse mass, so that energy and mass are connected in a most intimate manner. In fact, changes in energy and changes in transverse mass are proportional, the factor of proportionality being the square of the velocity of light.

Thus we have enumerated several important and striking connections, brought to light by the theory of relativity, where we have heretofore supposed that there were no connections whatever. If we look at these a little more closely we shall be able to perceive a still more profound connection underlying those which we have already mentioned. A material body in motion has kinetic energy. The connections which we treated depend on relative motion. It is then not far to the conclusion that these connections depend essentially on the energy which is involved in this relative motion. Thus all the interlocking relations mentioned might be thought of as due to the energy states involved in the several cases.

[1] See Bumstead, American Journal of Science (4), 26 (1908), 501.

[2] See PHYSICAL REVIEW, Vol. I., Series 2, p. 176.

This idea seems to me to be one fruitful of important conclusions; but it cannot be taken up in detail at this place.

Less abstruse connections than those mentioned above are also being brought to light. Of these one only will be considered. Page[1] has shown how, on the basis of the theory of relativity, the fundamental relations of electrodynamics may be derived from those of electrostatics. That is to say, where in the classical theory we have the *two* subjects of electrostatics and electrodynamics to be developed separately we have in the theory of relativity the *single* subject which is the synthesis of these two. This synthesis is of an essential nature; it is not a juxtaposition of things which belong apart, but a real unifying of two formerly distinct subjects into one larger and unseparated whole.

This may be taken as a special case of the way in which the theory of relativity enables us to obtain the laws of a moving system from those of a system at rest. The further development of this branch of the subject, one would believe, will lead to important results.

In this way it is obvious that the theory of relativity will introduce a much greater unity into physical science; the value of such a unifying factor can hardly be overestimated.

From the foregoing considerations we conclude finally that the theory of relativity has already made and is yet making important contributions toward the great philosophical problem concerning the one and the many.

XI. On the Nature and Possibility of Experimental Proof of the Theory of Relativity.

There are at least two ways in which it may be possible to demonstrate experimentally the accuracy of the theory of relativity.

The first one consists in the direct proof by experiment of the postulates on which the theory is based. These proved, the whole theory then follows by logical processes alone. In my first paper referred to above I have already given a sufficient discussion of this method.

A second method would be as follows: Among the consequences of the theory of relativity seek out one which has the property that if it is *assumed* the postulates of relativity may themselves be then deduced by logical processes alone. If then this assumption is proved experimentally this is sufficient to establish the postulates of relativity, and hence the whole theory. Or, one may find such experimental results as lead to all the essential conclusions of relativity, whence one naturally concludes to the accuracy of the whole theory. A discussion of proofs of this kind is to be found in my second paper referred to above.

[1] American Journal of Science (4), 34 (1912), 57–68.

This indirect method of proof is open to an objection of a kind which does not obtain in the case of the direct method previously mentioned. In the indirect method some auxiliary law, as for instance the law of conservation of electricity, must usually be employed in deducing the relativity postulates or essential conclusions from the new assumption which one attempts to justify by experiment. There is always the possibility that the auxiliary law is itself wrong; and consequently one's confidence in the accuracy of the relativity postulates as thus deduced can be no stronger than that in the truth of the auxiliary law. The same objection can also be raised against many conclusions which we are accustomed to accept with confidence.

To many persons it appears that the first method of proof mentioned above has been carried out successfully and satisfactorily. But if one does not share this opinion it is still legitimate to accept the theory of relativity as a working hypothesis, to be proved or disproved by future experiment. It is an historical fact, patent to every student of scientific progress, that many of our fundamental laws have been accepted just in this way. Take, for instance, the law of conservation of energy. There is no experimental demonstration of this law; and in the very nature of things it is hard to see how there could be. On the other hand, it is at variance with no known experimental fact. Moreover, it furnishes us a very valuable means of systematizing our known facts and representing them to our minds as an ordered whole. In other words, it is the most *convenient* hypothesis to make in the face of the phenomena which we have observed. Similarly, even if one does not believe that the theory of relativity has been conclusively demonstrated, should he not accept that theory (tentatively at least) provided it furnishes him with the most convenient means of representing external phenomena to his mind?

Finally, it should be said that every supposed proof of the theory of relativity is of such character that objections can be raised to it; likewise, every supposed disproof of the theory is in the same state. In the mean time, though we cannot accept the theory with all confidence, we can at least use its conclusions to suggest experiments which otherwise would not have been conceived. Therefore, whether true or false, the theory will be useful in the advancement of science. On this account, if on no other, it should appeal to every person interested in scientific progress.

INDIANA UNIVERSITY.

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CHANGE OF INDEX OF REFRACTION OF WATER WITH CHANGE OF TEMPERATURE.

BY FREDERICK A. OSBORN.

INTRODUCTION.

THE change of the index of refraction of water with change of temperature has been studied by a number of investigators, among whom the following may be mentioned:

Jamin[1] made observations on the change of the index of refraction of water between 0° and 30°. He used an interference method, but apparently did not publish any values. He contented himself with a general statement of the change and gave the empirical formula

$$K_t = K_0 - (0.000012573t) - (0.000001929t^2)$$

as expressing his results.

Gladstone and Dale,[2] soon after the work of Jamin, studied the same problem, making use of a hollow prism and the method of minimum deviation. They worked between the temperatures of 0° and 40° and remark "our determinations were performed repeatedly and most carefully on water near the freezing point." They call attention to the fact that the change of index between 0° and 5° is much less than that between 5° and 10°, finding from their work that the change is .0001 and .0002 respectively. Their method enabled them to express the change of index to the fourth decimal place.

Ruhlman,[3] by a method similar to that of Gladstone and Dale, obtained the index of refraction of water between 0° and 100° for the lithium, sodium and thallium lines. His values are given to five decimal places.

Lorenz[4] used an interference method and worked between 0° and 34°. $d\mu/dt$ was found from the shift of the fringes with change of the temperature and an accuracy of 3 units in the seventh decimal place was claimed. No experimental data were given but his results are expressed by the equation

[1] Comptes Rendus, 43, p. 1191, 1856.
[2] Phil. Trans., p. 887, 1885.
[3] Pogg. Ann., 132, p. 1177, 1867.
[4] Wied. Ann., 11, p. 70, 1880.

$$\frac{d\mu}{dt} = -10^{-6}(0.076 - 5.606t + 0.06403t^2) \text{ for the Na line,}$$

$$= -10^{-6}(0.952 - 5.586t + 0.06402t^2) \text{ for the Li line.}$$

Dufet[1] used two different methods. In the prism method, the prisms filled with water at a given temperature, were adjusted to minimum deviation. The water was then exchanged for water at a higher temperature, and the displacement of the lines was observed as the temperature fell. From this shift of the lines, and the other measurable quantities, he computed $d\mu/dt$. In the interference method he used Talbot's fringes, and observed the passage of about 180 fringes between 0° and 50°. In part of this work a quartz plate was immersed in the water, and for part a crown glass plate was substituted. The values for $d\mu/dt$ obtained by his different methods sometimes differ from each other by as much as 45×10^{-7}. Dufet expresses his results by the equation

$$\frac{d\mu}{dt} = -10^{-7}(125.46 + 41.285t - 0.01304t^2 - 0.0046t^3).$$

Values for $d\mu/dt$ obtained from this equation occasionally differ from his mean experimental values by as much as 7×10^{-7}.

Kettler[2] repeated Ruhlman's work for temperatures above 20° using a total reflection refractometer.

Walter,[3] using the minimum deviation method, has done some careful work for the range 0° to 30°. His results are given to the fifth decimal place.

Conray,[4] by the same method, but with a spectrometer graduated to 10″, has made a study of the change of index between 0° and 10°. He claims an accuracy in the sixth decimal place, although greater differences between successive determinations for the same temperature range are found.

Bender,[5] with the Pulfrich refractometer, has studied the change of the index of refraction of water between 0° and 40° using the H_α, H_β, H_γ lines. His values are expressed to five decimal places.

More recently Gifford,[6] using a new prism method, has determined the index at 15° for a large number of wave-lengths. The values obtained are considered correct to within two units in the fifth decimal place. A

[1] J. de Phy., p. 401, 1885.
[2] Wied. Ann., 33, p. 353, 506, 1888.
[3] Wied. Ann., 46, p. 422, 1892.
[4] Proc. Royal Soc., 58, p. 228–234, 1895.
[5] Ann. d. Phy., p. 343, 1899.
[6] Roy. Soc. Proc., 70, p. 329, 1902; 78, p. 406, 1906–7.

still later determination by Baxter, Burgess, and Daudt[1] gives the value of the index for Na light at the temperatures 20°, 25°, and 30°. These investigators used the prism method also and consider their results correct to three units in the fifth decimal place.

While the change of the index of refraction of water with change of temperature has been studied by these and many others, the methods used and the results obtained are such as to make it seem desirable to repeat the work.

It was thought that the Abbe-Pulfrich interferometer could be made to give more accurate results than those thus far obtained and the present paper has for its object the application of the interferometer to the determination of the change of the index of refraction of water with change of temperature.

METHOD.

The method used in this piece of work is that first used by Reimerdes[2] in his work on the change of the index of refraction of quartz. The change in the index of the liquid is determined by the shift of interference fringes in a fused quartz system.

Fig. 1.

The arrangement of the system is shown in Fig. 1. A bed plate *A*, of fused quartz, 38.4 mm. in diameter and 10 mm. thick, with its upper face plane polished, is placed on a horizontal surface. Upon the quartz surface is placed a fused quartz ring *Q* of 31.8 mm. in external diameter, 24.4 mm. internal diameter, and approximately 9.9 mm. high. The cover plate *B* of fused quartz, is approximately 36 mm. in diameter and 8 mm. thick. Its surfaces are plane polished and inclined to each other at an angle of about 20″, so as to deflect from the field the light reflected from its upper surface.

The quartz ring was cut from a plate of quartz whose surfaces were plane polished and slightly inclined to each other. The ends of this ring were ground away in such a manner as to leave on each end three equidistant feet, whose surfaces are portions of the original plane polished surfaces of the plate from which the ring was cut. The inclination of the faces of this plate to one another was such that when the ring is placed, as in the figure, between the bed and cover plates the opposite reflecting surfaces, *i. e.*, the upper surfaces of the bed plate and the lower surface of the cover plate, are properly inclined to produce suitable interference fringes.

[1] Am. Chem. Soc. Jour., 33, p. 893, 1911.

[2] Inaugural Dissertation, Jena, 1896.

The cover plate B has on its lower surface at the center, a small circle which serves in the field of view as the reference point from which the measurements are made.

When the apparatus is in adjustment and the field illuminated by monochromatic light, the field is seen to be crossed by parallel interference fringes. Each dark band represents a definite thickness of the air between A and B. If λ be the wave-length of the light used, then the difference in the thickness of the air layer for any two adjacent dark bands is $\lambda/2$. If the space between A and B be filled with a liquid, the effect is to increase the optical thickness between A and B from d to μd and the distance between any two fringes, the band width, is less than the distance between these same fringes in air.

When the fused quartz system and the liquid are heated, the fringes move across the field toward the thick edge of the wedge. The index of refraction of the water decreases with rise of temperature, hence the wave-length in the water becomes longer. There will be fewer wave-lengths between any two opposite points of the wedge. But a given fringe is due to the retardation of a definite number of wave-lengths of the light reflected from the upper surface of the bed plate over that reflected from the lower surface of the cover plate, so if the wave-lengths become longer, a thicker part of the wedge is needed to give the same number of wave-lengths retardation. Therefore, the bands move toward the thick edge of the wedge and the number passing the reference circle indicates the decrease in the number of waves between the surfaces at that point due to the lengthening of λ.

The expansion of the quartz ring increases the distance between the plates and decreases the number of fringes that would otherwise cross the reference circle toward the thick edge of the wedge, or increases the number of wave-lengths under the reference circle, which would cause a shift of the fringes toward the thin edge of the wedge. The resultant shift of fringes is the difference of these two effects and, owing to the small expansion of the fused quartz, is toward the thick edge of the wedge.

The change in the index of refraction for a given temperature change is given by the formula

$$\frac{d\mu}{dt} = -\frac{N\frac{\lambda}{2}}{L_1(t_2 - t_1)} - \alpha\mu_1.$$

That this is so appears from the following considerations.

Let n be the number of fringes shifted for the temperature interval $(t_2 - t_1)$.

L_1 and L_2 be the thicknesses of the liquid (length of ring) at t_1 and t_2.
t_1 and t_2 be the lower and higher temperatures.
μ_1 and μ_2 be the indices of refraction of the liquid at t_1 and t_2.
λ be wave-length in air of the light used.
λ_1 and λ_2 be corresponding wave-lengths in the liquid at t_1 and t_2.
α be the mean coefficient of expansion of the fused quartz ring for the given temperature interval $(t_2 - t_1)$.

$$\mu_1 = \frac{\lambda}{\lambda_1} \text{ and } \mu_2 = \frac{\lambda}{\lambda_2}, \quad \lambda_1 = \frac{\lambda}{\mu_1} \text{ and } \lambda_2 = \frac{\lambda}{\mu_2},$$

$$n = \frac{2L_1}{\lambda_1} - \frac{2L_2}{\lambda_2} = \frac{2L_1}{\frac{\lambda}{\mu_1}} - \frac{2L_2}{\frac{\lambda}{\mu_2}} = \frac{2L_1}{\frac{\lambda}{\mu_1}} - \frac{2L_1[1 + \alpha(t_2 - t_1)]}{\frac{\lambda}{\mu_2}}$$

$$= 2L_1 \frac{(\mu_1 - \mu_2)}{\lambda} - \frac{2L_1}{\lambda} \mu_2 \alpha (t_2 - t_1),$$

$$\mu_2 - \mu_1 = -\frac{n\frac{\lambda}{2}}{L_1} - \mu_2 \alpha (t_2 - t_1),$$

$$\frac{d\mu}{dt} = \frac{\mu_2 - \mu_1}{t_2 - t_1} = -\frac{n\lambda}{2L_1(t_2 - t_1)} - \mu_2 \alpha$$

$$= -\frac{n\lambda}{2L_1(t_2 - t_1)} - \mu_1 \alpha,$$

as the difference between μ_1 and μ_2 is not sufficiently large to affect the value of the corrections.

APPARATUS.

The instrument used was an Abbe-Pulfrich interferometer such as described in detail by Pulfrich[1] and also by Randall.[2] The source of the light was a small mercury arc lamp, giving a very brilliant light in which the line in the green was especially prominent.

The quartz system was set up in a glass cell, 7 cm. in diameter and 4 cm. deep, which was placed in the double walled galvanized iron tank, *T*. The cover, *G*, closed the tank and made it water-tight by means of mercury between the double walls. The small opening in *G* at the top, 3 cm. in diameter, was closed by a glass plate. A Beckmann thermometer, *B*, and a stirrer, *S*, were introduced into the cell through the cover as shown in Fig. 2. The tank with its optical system was rigidly supported by the arm *L*, which was clamped to the stone pier on which the interferometer rested.

[1] Zeitschr. fur Instrumentenkunde, 1898, p. 261.
[2] PHYS. REV., 20, 1905.

The heating was obtained by means of two coils of No. 28 iron wire wound on the insulated vessel V, which was 18 cm. in diameter and 25 cm. deep. The coils, shown by the small circles, were wound separately, the turns about 5 mm. apart, so that the coils could be used alone, in series, or in parallel. The coils were covered with a layer of asbestos and then with wool felt. This vessel was made to fit closely into another iron vessel which was set inside a larger tank 35 cm. in diameter and 40 cm. high. The space between the two could be filled with water or with ice.

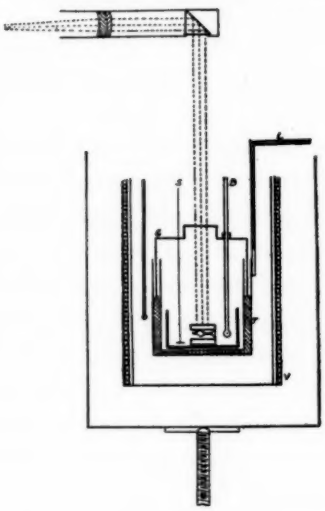

Fig. 2.

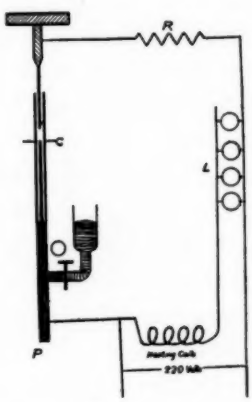

Fig. 3.

The thermostat used was a slight modification of a form which has already been described by the writer.[1] Around the inside of V was a liquid thermometer of 180 c.c. volume. This thermometer consisted of a ring of glass tubing, 17 cm. in diameter with six thin-walled vertical glass tubes, 20 cm. long, fused into it and hanging downward, all filled with toluol, except one which was filled with mercury and connected with the regulator at P, Fig. 3.

Fig. 3 shows the regulator and the electrical connections. The heating current was regulated by a lamp bank, L, not more than six 16 c.p. 220-volt lamps being required for the highest temperature. The thermostat was adjusted for a given temperature, by making the resistance in the rheostat, R, of such value that the current in the heating coils was insufficient to raise the temperature of the bath; the latter, cooling slightly, caused the mercury in the capillary, C, to fall, thus cutting out the rheostat circuit, and allowing more current to go through the heating

[1] Jour. Phys. Chem., 9, 1905, p. 297.

coils. When the temperature of the bath rose, the mercury rose also in the capillary, cutting in the rheostat.

Since the large volume of the liquid thermometer made it more sensitive than the thermometer in V, the bath was easily held at a temperature, constant to within 2 or 3 thousandths of a degree for half a day or more. The consequent variation of the temperature in the cell was not more than .002° in the same ime.

Stirrers in T and V kept the water thoroughly agitated.

ADJUSTMENT OF APPARATUS.

The preliminary adjustments of the instrument and the optical system for work with the fringes has been described by Pulfrich.[1]

When the system was in good adjustment, the liquid whose index of refraction was to be studied was poured into the glass cell, until, when the cover plate B was put on the quartz ring, the liquid came within a mm. of the top of B. The cover G was then put on and the entire bath raised by means of a screw until the water around the tank T came within a cm. of the glass top on G. The temperature of the bath was then raised to the desired point and the thermostat set in operation by closing the stop cock O and adjusting the platinum point in the capillary tube.

The temperature in the cell would gradually rise for two or three hours, after which the Beckmann thermometer showed no change of temperature greater than .002°. When the temperature had been constant for two hours longer, the position of the bands was read and also the temperature. One hour later both were read again. If no change in either was found, the temperature of the liquid was assumed to have become constant.

The temperature was then slowly raised 5° and the displacement of the fringes was counted. This displacement was, after the first 10 minutes, very uniform, until the temperature of the cell was within 0.5° of that of the surrounding water bath when the displacement became slow. The entire time for this displacement of the fringes was usually about two hours. But, as in getting the initial position of the bands, the final reading was not taken until the thermometer had been constant to within .002° for three hours. The number of whole bands shifted being known by actual count, the total shift was then given by the formula.

$$N = M + d_2 - d_1,$$

where M = number of whole bands counted, d_2 and d_1 the fractions of a band at the higher and lower temperatures respectively. These fractions were found from the readings of the micrometer with which the interferometer was provided.

[1] Zeitsch. fur Instrumentenkunde, 13, Oct., Nov., Dec., 1893.

Thermometers.

Two thermometers were used: For the water bath, thermometer No. 2,107, range − 10° to 50° by 1/10°, recently calibrated at the National Bureau of Standards; for the liquid in the cell a Beckmann thermometer, compared with No. 2,107, and, for the 5° interval, compared with one made by Haak, reading to .01° which had been calibrated at the Reichsanstalt.

The mean temperature of the water in the cell was obtained in each case by translating the readings of the Beckmann thermometer into degrees centigrade.

Spherometer.

A Geneva Society spherometer was used to measure the quartz ring, and the readings could be made to .001 mm. A mean of thirty readings gave for the length of the ring 9.901 mm.

Degree of Accuracy.

From the formula

$$\frac{d\mu}{dt} = -\frac{n\lambda}{2L_1(t_2 - t_1)} - \alpha\mu_1$$

it is evident that the value of $d\mu/dt$ depends upon the three measurable quantities, n, L, and t.

In finding n from the formula

$$n = M + d_2 - d_1,$$

the d's can be found with an error affecting n by ± .02 of a fringe. The resulting error in $d\mu/dt$ is 10×10^{-8}. The difference in temperature could be determined with an error of ± .002. The error in $d\mu/dt$ from this source is 4×10^{-8}. The length of L could be found to ± .001 mm., resulting in an error in $d\mu/dt$ of 1×10^{-8}.

The total error in the final result due to these errors is

$$E = 10^{-8}\sqrt{10^2 + 4^2 + 1^2} = 10.8 \times 10^{-8},$$

or 1 unit in the seventh decimal place.

Scheel has found the length of a fused quartz ring similar to the one used here to be well represented by the expression

$$l_t = l_0(1 + 0.322 \times 10^{-6}t + 0.00147 \times 10^{-6}t^2).$$

The mean value of α computed from the above formula for each temperature range was used; and μ_1, as found by the prism method, using a Zeise spectrometer, is 1.3342. μ_1 need not be known with the greatest accuracy as an error of .001 is negligible as affecting the factor $\alpha\mu_1$.

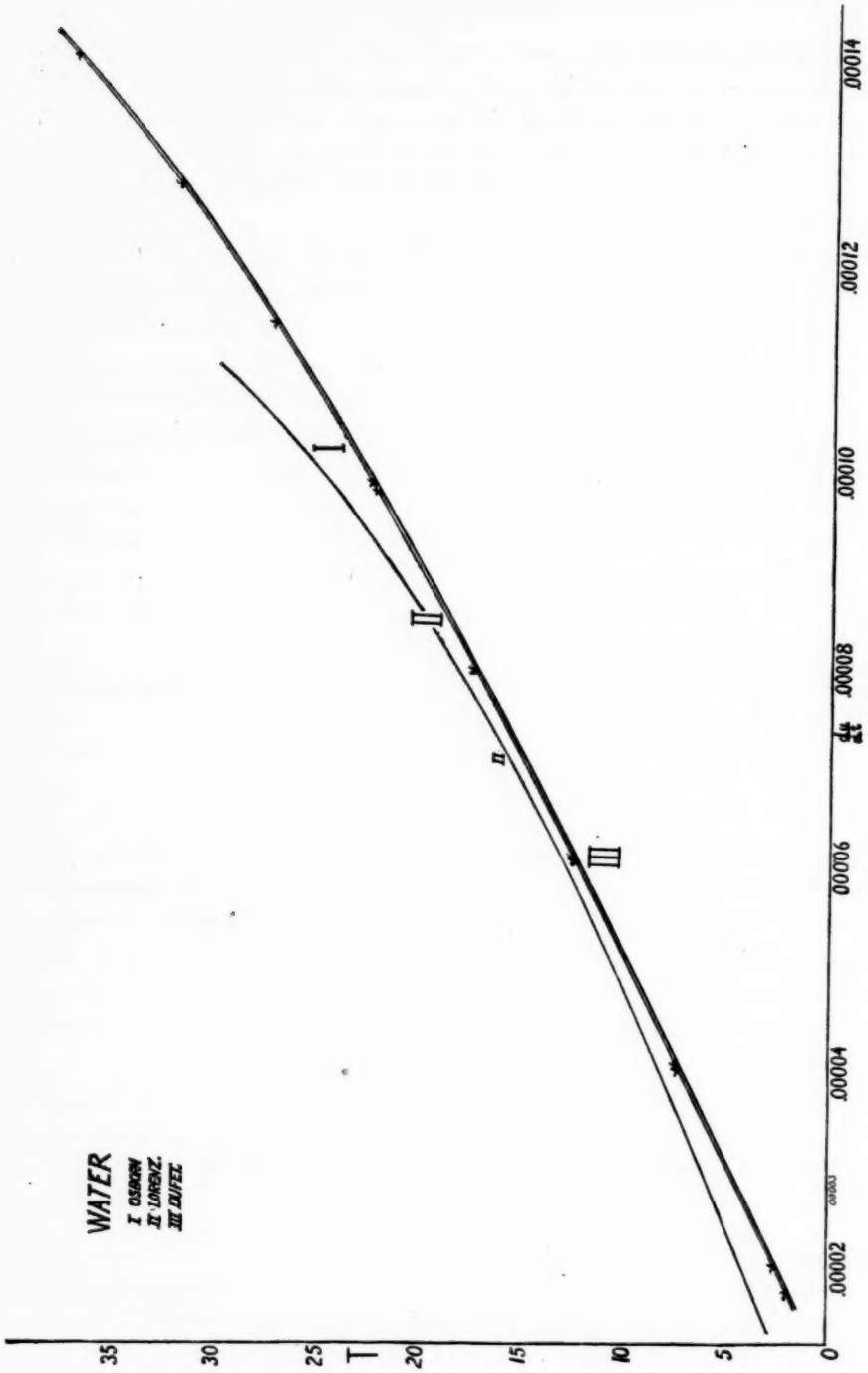
WATER
I OSBORN
II LORENZ
III DUFET
I
II
III
T
35
30
25
20
15
10
5
0
.00002
.00004
.00006
.00008
.00010
.00012
.00014

Fig. 4.

Example of a Complete Determination.

Length of fused quartz ring.....9.901 mm.
Thickness of water layer.....9.901 mm.
Conductivity of water used (free from dissolved air).....1.7×10^{-6}

Time.	Temperature of Bath.	Temperature of Water in the Cell, Beckmann Thermometer.	Temperature of Emergent Column of Beckmann.	Shift of Fringes.	d's from Micrometer Readings.
9:30A.M.	25.00° C.	1.150			12 noon
1:00		.170	23.5	0	
2:00		.190			$d_1 = .190$
1:00		.190			1:30 P. M.
1:30		.190			$d_2 = .186$
					Mean .188
1:45	25.5			1	
50		1.900		1	
54				1	
58				1	
2:02				1	
06				1	
10				1	
14				1	5:00 P. M.
18				1	
22				1	
26				1	$d_2 = -.047$
29				1	
33				1	
37				1	
40				1	6:30 P. M.
44				1	
48				1	$d_2 = -.043$
54				1	
59				1	
3:16	29.98			1	Mean $-.045$
4:00		6.120	25.5	1	
5:00		.130		21	
30		.130			
6:30		.130			

$$N = M + d_2 - d_1 = 21 + (-0.045) - 0.188 = 20.767,$$

$$t_2 - t_1 = 6.130 - 1.190 = 4.940°,$$

$t_2 - t_1$ corrected for emergent stem and mercury out of bulb = 4.999°,

$$\frac{d\mu}{dt} = -\frac{20.767 \times .0005460}{2 \times 9.901 \times 4.999} - .00000048 = -.0001150$$

at 27.49°.

TABLE I.

Water.

Conductivity of water, 1.7×10^{-6} (air-free).
$L = 9.901$ mm.
$\lambda = .0005460$ mm.

Date.	Series.	Temp. Range.	Temp. Diff. $t_2 - t_1$	Total Shift of Fringe.	$\frac{d\mu}{dt}$	At
March 23	1	20–25	5.014	17.95	.0000992	22.58°
25	1	20–25	4.992	17.90	983	22.33
26	2	25–30	4.999	20.767	1150	27.49
27	2	30–25	4.981	20.684	1150	27.51
28	2	30–35	4.952	22.978	1284	32.25
28	2	35–30	4.860	22.540	1284	32.23
April 2	2	35–40	5.015	25.541	1409	37.50
3	2	40–35	5.013	25.528	1409	37.50
5	3	15–20	4.934	14.393	809	17.37
6	3	20–15	4.860	14.156	808	17.38
6	3	15–10	4.965	11.170	625	12.49
8	3	10–5	4.990	7.617	424	7.49
8	3	5–0	4.714	3.805	227	2.61
11	4	10–15	5.009	11.284	624	12.50
11	4	15–20	5.007	14.607	809	17.37
12	4	20–25	4.961	17.786	993	22.53
12	4	25–30	4.893	20.331	1150	27.46
13	4	30–35	5.052	23.429	1284	32.27
13	4	35–40	4.910	25.017	1410	37.48
14	4	5–10	4.730	7.271	420	7.34
14	4	5–0	4.495	3.632	227	2.59

Table I. gives the complete set of experimental data. Each series represents a new set-up of the apparatus and a new supply of water taken from a common source.

Fig. 4, Curve I., expresses the results graphically, and they may also be given by the equation

$$\frac{d\mu}{dt} = -10^{-7} \times (118.73 + 41.4184t - 0.02376t^2 - .0043757t^3).$$

This equation expresses correctly the experimental results to approximately one unit in the seventh decimal place.

CONCLUSION.

The results contained in Table I. show that the method here employed gives more concordant results than any method previously used and that in the determination of $d\mu/dt$ an accuracy of one unit in the seventh decimal place has very approximately been attained. This result has been achieved principally through the accuracy with which the shift of the

Table II.

Comparison of Results.[1]

Temp.	Lorenz.	Dufet.	Osborn.	Temp.	Lorenz.	Dufet.	Osborn.
38°		.0001423	.0001418	19°	.0000833	.0000874	.0000867
37		1402	1398	18	801	838	831
36		1380	1375	17	767	801	794
35		1357	1352	16	732	764	757
34		1333	1327	15	696	726	720
33		1308	1301	14	659	688	682
32		1282	1276	13	620	650	644
31		1256	1249	12	580	611	605
30	.0001105	1228	1221	11	538	572	565
29	1087	1200	1194	10	496	532	526
28	1067	1170	1163	9	452	493	486
27	1045	1140	1134	8	407	453	447
26	1024	1109	1103	7	360	412	406
25	1001	1078	1072	6	313	372	365
24	976	1045	1039	5	264	331	325
23	950	1012	1006	4	213	290	284
22	923	978	972	3	162	249	243
21	894	944	938	2	109	208	201
20	864	909	903	1	55	167	160

interference bands could be measured by the Abbe-Pulfrich interferometer and also by the extremely exact control and measurement of the temperature.

In comparing the results here obtained with those of other observers, it will be noticed from the results tabulated in Table II. and also from the Curves I. and III. that a constant difference exists between the results of Dufet and those of the writer. This difference is six or seven units in the seventh decimal place. The experiments of Lorenz furnish data for estimating the variation of $d\mu/dt$ with respect to λ. The change in $d\mu/dt$ to be expected for a change from the Na line, λ 5,893, used by Dufet to the Hg line, λ 5,460, used by the writer, is, according to Lorenz, 6.8 units in the seventh decimal place in the temperature region 10°–30°. The results of the two investigations appear therefore to be in complete accord. This is somewhat remarkable when one considers that each of Dufet's final results is the mean of four different determinations which often differ from one another by several units in the sixth decimal place.

The empirical equation here given expresses the experimental results to one unit in the seventh decimal place. It is given in preference to adopting the Dufet equation with a suitable alteration of its constant term as it agrees more closely with the experimental data.

[1] Computed from the empirical equations.

Gifford's data on the change of the temperature refractive coefficients with wave-length are not sufficiently explicit to warrant an estimate of the difference which should exist between the values here presented and those of Dufet.

The changes in the index for the temperature interval 20° to 25° and 20° to 30° as computed by the formulæ of Dufet and the writer, the later values being corrected for wave-length, *i. e.*, to Na 5,893, agree with the recently measured changes according to Baxter, Burgess, Doudt, as the following table shows.

	B., B. & D. Observed.	Dufet Comp.	Osborn Comp.
20°–25°	.00051	.0004973	.0004975
20°–30°	.00109	.0010745	.0010748

The differences all lie within the estimated errors of observation.

To Professor Reed, under whose direction this work was done, the writer wishes to express his thanks.

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ON THE ELECTRICAL NATURE OF COHESION.

By Fernando Sanford.

ONE of the most direct methods of measuring cohesion between a dissolved substance and its solvent is by means of volume changes in the solvent. This method has been used with great success by Heydweiller in a series of investigations, the results of which he has published in the Annalen der Physik.[1] In these papers Heydweiller has determined for a large number of the chemical elements a cohesion constant by means of which he is able to calculate the cohesion between water and compounds of these elements. This constant he calls the *Ionenmodulus*. It is calculated in various ways according to the assumptions made in regard to the relative change in the volume of the water and the dissolved substance. In the one here used, which he indicates by the symbol A_s, the modulus represents the percentage increase of the density of the water in which the ions are dissolved. The numbers are extrapolated for infinite dilution from curves plotted for various concentrations, so that complete dissociation may be assumed. It is also assumed that the ions, themselves, occupy the same volume which they occupied in the solid or liquid state before solution.

The values of A_s used in this paper are the final values adopted by Heydweiller, and are to be found in *Tabelle* 30[1], Ann. d. Phys., 37, p. 765, 1912.

I have already referred in several papers to an electrical constant which I have called the characteristic charge of the ion, and which is calculated by multiplying the mobility of the ion as determined in electrolysis by its ionic mass. I wish to show in the present paper that this electric constant of the ion is proportional to Heydweiller's *Ionenmodulus*.

If m = the mass of the ion and u = its mobility, Heydweiller's *Ionenmodulus* may be calculated from the equation $A_s = muk$, where k = .00112 for univalent ions and .00372 for divalent ions.

In the following table the mobility of the ions is taken from Kaye and Laby's Tables, p. 88. The velocities taken are those actually observed, *i. e.*, one half the numbers usually taken in the case of the divalent ions.

[1] A. Heydweiller, Ann. d. Phys., 30, p. 873, 1909; 33, p. 145, 1910; 37, p. 739, 1912.

If the usual numbers for the mobility of divalent ions be used, k becomes .00186 for these ions.

TABLE I.

Ion.	Mobility.	Ionic Mass.	muk.	A_s.
	k = .00112			
Li	34.6	7	.27	.35
Na	45.2	23	1.31	1.38
K	67	39.1	2.34	2.10
Rb	70.5	85.45	6.9	6.32
Cs	70.5	132.81	10.50	10.58
NH_4	66.3	18	1.33	.98
Cl	67.8	35.46	2.70	3.02
Br	70	79.92	6.28	6.68
I	68.8	126.92	9.80	10.27
OH	180	17	3.46	3.40
NO_3	64	62	4.68	4.54
$C_2H_3O_2$	42.1	59	2.78	3.04
	k = .00372			
H	330	1	1.22	1.05
F	48.3	19	3.42	3.16
Mg	23.8	24.32	2.17	2.66
Ca	26.8	40.09	4.00	4.04
Sr	26.8	87.62	8.74	8.76
Ba	28.7	137.37	14.66	13.08
Cd	24.6	112.40	10.30	10.86
Pb	31.7	207.1	24.4	20.68
Cu	24.5	63.57	5.79	7.26
Zn	24.2	65.37	5.88	7.22
Ag	56	107.88	22.46	10.02
SO_4	35.5	96	12.65	11.54

It will be seen that the values of A_s calculated in this way differ from the observed values by less than the probable error of determination except in a few cases. Hydrogen and fluorine are placed with the divalent ions, though they undoubtedly belong in the monovalent group, and silver, copper and zinc give too low values of A_s. Silver is placed with the divalent ions in the table, and it will be seen that with the divalent constant it gives A_s twice as high as it should be. On the other hand, it gives too small a value with the monovalent constant. For copper and zinc the values of mu should be multiplied by the sum of both constants to give Heydweiller's values of A_s.

Notwithstanding these discrepancies, which will probably be explained later, the evidence seems conclusive that the quantity mu, which I have called the characteristic charge of the ion, is a determining factor in cohesion.

The relations brought out in Table I. are shown graphically in Fig. 1, in which Heydweiller's values of A, are plotted against the ionic charges, mu. Here the values of u for the divalent ions are those which are generally used, and are twice as great as those used in the table.

If the product um represents an electric charge, then in the case of the negative ions this charge must be negative, while in the case of the

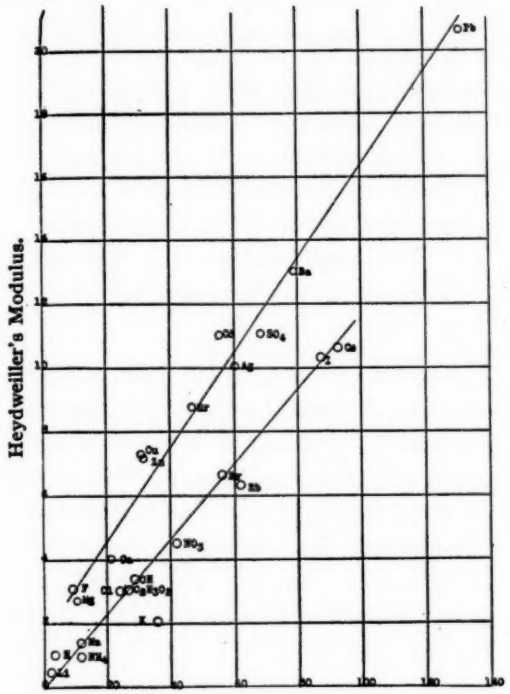


Fig. 1.

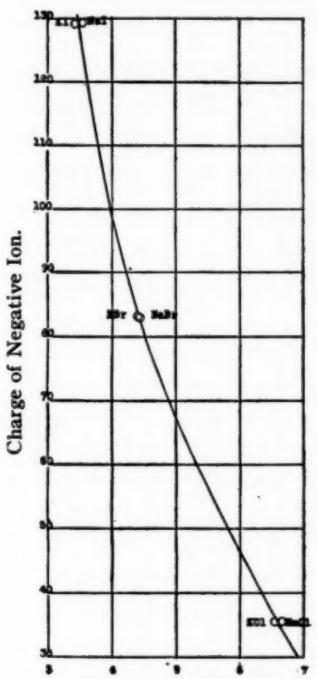


Fig. 2.

hydrogen and metallic ions it is positive. It will be seen from the above data that the attraction of the dissolved ions for water is practically independent of the character of the charge. In chemical combinations, however, the negative ion seems to play the principal part in determining cohesion. This is shown in Fig. 2, in which the specific cohesion of the sodium and potassium salts of chlorine, bromine and iodine are plotted against the charges of the negative ions as calculated from their mobility, assuming the hydrogen charge as + 4.84. The specific cohesion was determined by Motylewski by the drop method, using melted salts, and is quoted by Smiles (Chemical Constitution and Physical Properties, p. 40) from Zeit. Anorg. Chem., 38, 410, 1904.

It will be seen from the curve that the specific cohesion of the corre-

sponding sodium and potassium salts is nearly the same, the sodium salt regularly having the higher cohesion. If the mean cohesion of the corresponding sodium and potassium salts be taken, it may be calculated from the formula $C = 630/(e + 60)$, where C represents Motylewski's values for the specific cohesion and e = the charge of the negative ion. The values calculated in this way are compared with the observed values in the table below.

TABLE II.

Salt.	e	C Obs.	C Calc.
NaCl.....	35.2	6.63	6.59
KCl.....	35.2	6.55	
NaBr.....	82.8	4.44	4.43
KBr.....	82.8	4.41	
NaI.....	129	3.53	3.34
KI.....	129	3.44	

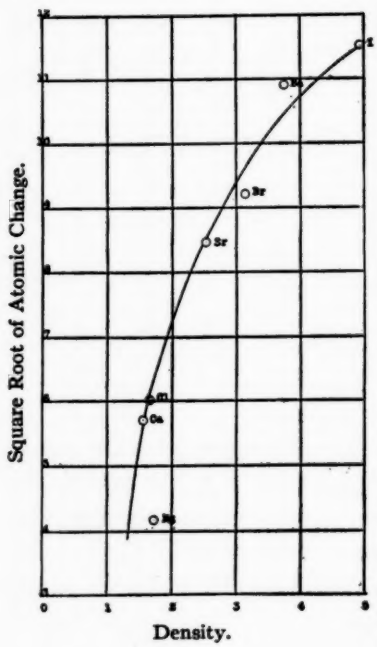


Fig. 3.

Another property which might reasonably be expected to vary with cohesion is density. When the density is compared with the atomic charge it is found that within a given group the density varies approximately as the square root of the atomic charge. If the usual numbers for the mobility of the ions of the barium group be used in calculating their atomic charges, the density of the elements of this group varies with the square root of the atomic charges at the same rate as does the density of the negative elements of the halogen group. This fact is shown graphically in the curve in Fig. 3, where the densities of the elements in the solid or liquid form are compared with the square roots of their atomic charges. Magnesium is the one element which is conspicuously out of place in this curve. Its density is proportionally greater in comparison with its other properties than that of any other element of its group.

The data from which this curve is drawn are given in Table III.

TABLE III.

Element.	Density.	$\sqrt{e}$.	Element.	Density.	$\sqrt{e}$.
F.....	——	3.8	Mg.....	1.74	4.16
Cl.....	1.66	6.04	Ca.....	1.58	5.7
Br.....	3.15	9.2	Sr.....	2.54	8.46
I.....	4.95	11.5	Ba.....	3.75	10.9

When two elements of the above groups combine to form salts, the specific gravities of the salts are approximately proportional to the products of the square roots of their atomic charges. Thus if e be the atomic charge of the positive ion and e' be the charge of a single atom in the negative ion, the specific gravity of these salts may be roughly calculated from the equation $d = k\sqrt{2ee'} + 1.5$, where $k = .022$. The observed values and the values calculated from this equation are given below.

TABLE IV.

Salt.	$\sqrt{2ee'}$.	d Calc.	d Obs.
$MgCl_2$.....	35.5	2.28	2.18
$CaCl_2$.....	48.7	2.57	2.26
$SrCl_2$.....	72.3	3.09	3.05
$BaCl_2$.....	93.2	3.55	3.85
$CaBr_2$.....	74	3.13	3.32
$SrBr_2$.....	109.7	3.90	3.96
$BaBr_2$.....	141.5	4.62	4.78
CaI_2[1].....	92.8	3.54	4.9
SrI_2.....	137.4	4.51	4.41
BaI_2.....	177	5.40	4.92

The fact that the above values are only rough approximations to the true values is explained by the curve in Fig. 4, where it is seen that the relation between the density and the products of the square roots of the atomic charges is not accurately represented by a straight line.

The fact that the specific gravities of these, and other, salts are related to the charges of their ions may be shown in another way. Thus, if the algebraic sum of the positive and negative ions of a molecule be taken, it should give a resultant molecular charge which may be either positive or negative. When these resultant molecular charges with their appropriate signs are plotted against the specific gravities, the salts are found to be

[1] The density of CaI_2 is clearly out of relation to the other salts in this table. It seems to have been determined but once, viz., by Ruff and Plato, Berliner Berichte, 35, and I will give further reasons in this paper for thinking that there is an error in the determination.

arranged in their appropriate groups with respect to both their positive and negative ions. In Fig. 5, the specific gravities of the chlorides,

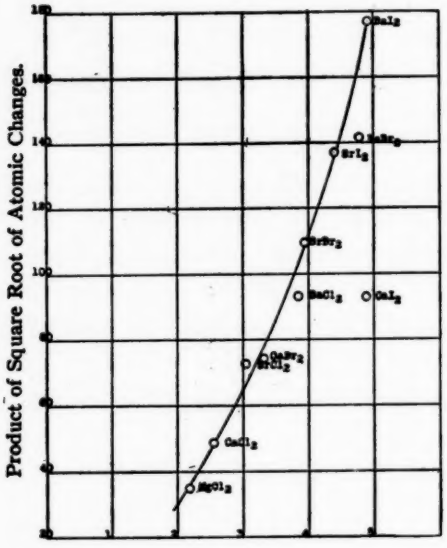


Fig. 4.

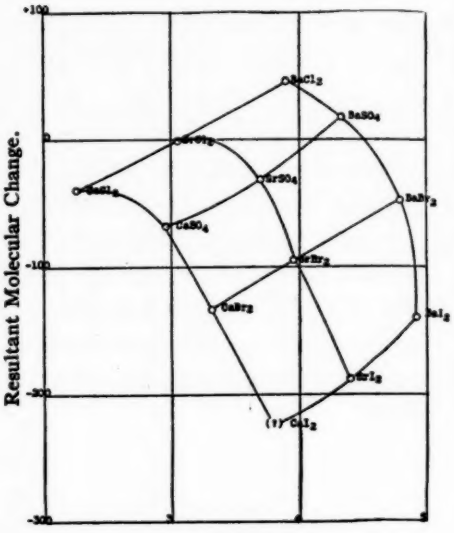


Fig. 5.

bromides, iodides and sulphates of calcium, strontium and barium are plotted as abscissas and their resultant molecular charges as ordinates.

It will be seen from this diagram that Ruff and Plato's value (4.9) for the specific gravity of CaI_2 is entirely out of relation to that of the other salts. I have accordingly ventured to give to this salt the provisional value 3.8, which it would seem to require from Fig. 4.

A closely analogous diagram to Fig. 5 is shown in Fig. 6. It seems to be a general law that those salts which produce the greatest volume contraction on solution in water give solutions which have the highest temperature expansion coefficients. The expansion coefficients used in Fig. 6 are given in the table below along with the specific gravities and the resultant molecular charges of the same salts. The expansion coefficient of the salt is determined by subtracting the expansion of water from that of the salt solution for the same temperature range. The increase in volume is for the interval between 19.5 degrees and 40 degrees, and is taken from Landolt and Boernstein's Tables. The molecular concentration is 20 gram equivalents of the salt in 10,000 grams of water. The increase in volume is given in hundred-thousandths of the volume taken at 19.5 degrees.

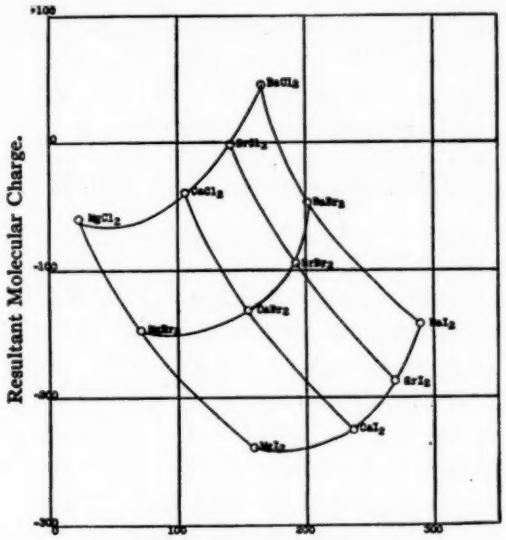


Fig. 6.

TABLE V.

Salt.	Resultant Charge.	Specific Gravity.	Expansion Coefficient.
$MgCl_2$	− 54.2		22
$CaCl_2$	− 39.6	2.26	105
$SrCl_2$	− 2	3.05	142
$BaCl_2$	+ 44.8	3.88	165
$MgBr_2$	−147.6		70
$CaBr_2$	−133	3.32	153
$SrBr_2$	− 95.4	3.96	182
$BaBr_2$	− 48.6	4.78	201
MgI_2	−240		158
CaI_2	−225	3.8(?)	236
SrI_2	−187.8	4.41	268
BaI_2	−141	4.92	288
$MgSO_4$	− 83		
$CaSO_4$	− 68.4	2.96	
$SrSO_4$	− 30.8	3.70	
$BaSO_4$	+ 16	4.33	

STANFORD UNIVERSITY,
November 9, 1912.

BROWNIAN MOVEMENTS IN GASES AT LOW PRESSURES.

BY R. A. MILLIKAN.

THE essential improvements which the "oil drop method"[1] introduced into the study of Brownian movements consisted in

1. Enabling the observer to hold a given particle under observation as long as desired, even in a rarified gas, by counteracting the downward pull of gravity by the upward pull of an electrical field.

2. Eliminating completely all uncertainty as to the resistance offered by the medium to the motion of the drop through it.

This last result was accomplished by combining the Brownian movement equation

$$\bar{D} = \sqrt{\frac{4}{\pi}\frac{RT}{NK}t}, \qquad (1)^2$$

in which R is the gas constant, T absolute temperature, N the number of molecules in a gram molecule, and K the unknown resistance factor, with the characteristic equation of the "oil drop method," viz.,

$$e = \frac{K}{F}(v_1 + v_2)_1, \qquad (2)^3$$

in which F is the electrical field strength, e the elementary electrical charge and $(v + v_2)_1$ the greatest common divisor of the series of values assumed by the sum of speeds $(v_1 + v_2)$ as the drop changes charge through the capture of ions. The resulting equation which gives the average Brownian displacement is

$$\bar{D} = \sqrt{\frac{4}{\pi}\frac{RT(v_1 + v_2)_1 t}{F(Ne)}}, \qquad (3)$$

[1] R. A. Millikan, PHYS. REV., XXXII., p. 349, April, 1911.

[2] This is merely the Einstein equation $\Delta x^2 = 2RT/NK \cdot t$ modified so as to give the average displacement instead of the average of the squared displacements by substituting from the Maxwell distribution law which holds for Brownian displacements as well as for molecular velocities $\bar{D} = \overline{\Delta x} = \sqrt{2/\pi}\sqrt{\overline{\Delta x^2}}$.

[3] This equation is merely equation 1, p. 353 (PHYS. REV., XXXII.), viz. $e_n = \frac{mg}{Fv_1}(v_1 + v_2)$ after $\frac{mg}{v_1}$ has been replaced by K (which is constant for a given drop) and after the whole equation has been divided through by n, the number of elementary charges in e_n.

an equation which differs from Brownian movement equations which preceded it in being completely independent of all assumptions as to the density or shape of the Brownian particle, or as to the law of motion of the particle through the medium. It contains nothing but accurately measurable elements and is actually found to yield under all circumstances calculated values of $\bar{D}$ which agree with observed values within the limits of experimental error (about 4 per cent. for the individual drops worked with, though only 0.6 per cent. for the mean of all the drops used).[1]

Another forward step in the study of Brownian movements becomes possible as soon as we become able to determine the constant K. This constant is defined by the equation

$$X = Kv_1, \tag{4}$$

in which X is the force acting on the particle and v_1 the velocity produced by this force. This step has now been taken by working with oil drops at all pressures from 5 mm. up by the methods already referred to (*l. c.*, 1, p. 1). The details of this work are shortly to be published in full, but the result which gives the law of motion of a drop of radius a through a gas of viscosity μ, and mean free path l[1] under the influence of a force X is contained in the empirical equation

$$X = 6\pi\mu a v_1 \left\{ 1 + \frac{l}{a}\left(.874 + .32\epsilon^{-1.5_4 \frac{a}{l}} \right) \right\}^{-1} \tag{5}$$

ϵ representing the Naperian base. This means that K of equations 1, 2, and 4, is given by

$$K = 6\pi\mu a \left\{ 1 + \frac{l}{a}\left(.874 + .32\epsilon^{-1.54 \frac{a}{l}} \right) \right\}^{-1}. \tag{6}$$

This makes it possible to determine precisely how the Brownian displacements $\bar{D}$ increase as the pressure decreases in the case of a drop of any given radius. A glance at equation (1) shows, since μ is independent of pressure, that

$$\bar{D} \propto \sqrt{1 + \frac{l}{a}\left(.874 + .32\epsilon^{-1.54 \frac{a}{l}} \right)}. \tag{7}$$

The accompanying table shows how $\bar{D}$ increases as the pressure falls from 760 mm. to 1 mm. in the case of the smallest drops (a = .00003 cm.) upon which accurate experiments have thus far been made in the use of the "oil drop method."

[1] Harvey Fletcher, PHYS. REV., XXXIII., August, 1911. R. A. Millikan, Popular Science Monthly, 80, p. 438, May, 1912; also Trans. Am. Electrochemical Soc., Vol. XXI., p. 185, April, 1912.

p mm. Hg.	l/a	A'	$\bar{D}/\bar{D}_0$
760	.314	.874	1.13
380	.628	.902	1.25
190	1.255	.968	1.49
100	2.385	1.042	1.88
50	4.770	1.106	2.50
25	9.540	1.142	3.45
10	23.85	1.174	5.39
5	47.70	1.184	7.58
2	119.25	1.190	11.95
1	238.5	1.192	16.89

The column headed A' in the table gives the variable values which must be assigned to the quantity A if the law of motion of a drop through a resisting medium is to be expressed in the form

$$X = 6\pi\mu a v_1 \left(1 + A' \frac{l}{a}\right)^{-1}. \tag{8}$$

A' is constant and has the value .874 so long as $l/a < .4$, after which it rises as shown on the table to the value 1.193, beyond which it never goes (see also equation 3). The column headed $\bar{D}/\bar{D}_0$ in the table gives the ratio between the Brownian displacement at the given pressure, and the displacement at a pressure so high that the term in l/a is negligible in comparison with unity. The values given in this last column are computed by means of (7) but all save the last three of them have been checked experimentally with no larger errors than those mentioned above.

The table brings out clearly the advance which the "oil drop method" introduced into the study of Brownian movements by making it possible to hold a given particle under observation for an indefinite time in a gas at any pressure from 1 mm. up. For example, the ratio between the Brownian displacement $\bar{D}_a$ of a given particle in air at atmospheric pressure where the term in l/a is nearly negligible and the Brownian displacement $\bar{D}_w$ of the same particle in water is

$$\frac{\bar{D}_a}{\bar{D}_w} = \sqrt{\frac{\mu_w}{\mu_a}} = \sqrt{\frac{.0110}{.00178}} = 7.7.$$

The displacement of the particle considered above is multiplied again 7.6 fold by reducing the pressure to 5 mm. of mercury (see table). The change from 760 mm. to 5 mm. corresponds then very closely to the transfer of the particle from water to air.

[1] l is computed from $\mu = .3502 mn\bar{c}l$, in which $\bar{c}$ is the average molecular velocity and $\mu_t = .0001824 + .00017856(t - 23°\text{ C.})$ .00276.

I have not yet carried the direct quantitative measurements to pressures lower than 5 mm., but I expect to do so in the near future and see no difficulties in the way of working at pressures as low as 1 mm. Under these conditions the Brownian displacements of the particle considered will be more than 100 times greater than they are in water, and the Brownian movement method of determining the elementary electrical charge should begin to be comparable in accuracy with the oil drop method, although its probable error would still be five or six times as great as that inherent in the latter method. The present value of e by this last method is

$$e = 4.774 \pm .009.$$

The value by the Brownian movement method fluctuates between Perrin's value 4.2×10^{-10} and Fletcher's value 5.01×10^{-10} with Svedberg's determination at the intermediate value 4.7×10^{-10}.

De Broglie,[1] who has made qualitative observation on the Brownian movement of smoke particles at low pressures, concluded that $\bar{D}$ was independent of pressure down to about 1 cm. of mercury, where it began to increase rapidly. This would be roughly true for particles ten times the size of those above considered, *i. e.*, for particles of radius $a = .0003$ cm. for the value of l/a at 1 cm. would then be 2.385 (see table) and $\bar{D}$ would be but $1.87/1.13 = 1.66$ times as much as at atmospheric pressure.

RYERSON LABORATORY,
THE UNIVERSITY OF CHICAGO,
December 20, 1912.

[1] De Broglie, C. R., 154, p. 112, January, 1912.

RECTIFYING PROPERTIES OF A PHOTO-ELECTRIC CELL.

BY S. HERBERT ANDERSON.

IT has long been known that a photo-electric cell in which electrons are freely emitted from a metal under the action of light has distinct rectifying properties. In some investigations by Professor Kunz and Dr. J. G. Kemp it was found that in a photo-electric cell of the type used by the author in the present investigation may be used as a detector of electric waves. When used in connection with a Fleming cymometer it was found much more sensitive than the neon tube furnished with the instrument. Since its use as a detector depends upon its rectifying property, the present investigation was undertaken to determine the adaptability to practical use in wireless telegraphy.

DESCRIPTION OF METHOD AND APPARATUS.

The form of the photo-electric cell used is shown in diagram by Fig. 1, *A*. One electrode is a hemispherical cap of pure potassium deposited in the lower part of bulb *a*, which is 4 cm. in diameter. The other electrode is a platinum point *b*, the distance of which from *c* can be adjusted by the action of a magnet on the iron ring *d*.

The method of preparing the potassium was the same described in a former paper.[1] The potassium collected in *e* was poured into bulb *a* and deposited on the lower half by distillation. It was desired to have the cell as stable as possible and to this end nitrogen was introduced into the tube *A*. For removing all traces of oxygen and water vapor from the nitrogen, tube *B*, Fig. 1, was used. Before the evacuation of the system a piece of potassium was introduced into the tube at *f* which was then sealed off. When the tube was evacuated the potassium was distilled so as to form a brilliant metallic surface all over the inside of *B*. With stopcock *g* closed, dry nitrogen was let into *B* through *h* at atmospheric pressure. The potassium near *h* turned black showing some oxidation. *h* was closed and the tube heated until again there was a bright metallic surface. This insured the removal of all oxygen from the gas. Then *g* was opened and nitrogen let into *A*. *g* was closed and the tube *A* pumped out until the discharge from a small induction coil passed

[1] PHYSICAL REVIEW, XXXV., p. 239, 1912.

easily between the electrodes *b* and *c*. Thus in a rough way the pressure of minimum sparking potential was obtained. When a potential of 220 volts of a 60-cycle alternating current was applied no discharge occurred. but with 330 volts a current of about 0.1 ampere passed. *A* was then sealed off from the pump at *k*.

The method used in examining the rectifying properties of the cell was the same as used by Pierce[1] in examining the crystal and electrolytic detectors and is shown in diagram by Fig. 2. The image of the luminous spot of the fluorescent screen of a Braun tube is brought to a focus on a drum *d* by means of a lens *l*. This drum is connected directly to synchronous motor which operates on a 110-volt, 60-cycle alternating current. The drum is covered with a sensitive photographic film and is enclosed in a light-tight box. The source of the current sent through the photo-electric cell *A* was a 440-volt, 60-cycle alternating current supplied by the same dynamo that furnished the 110-volt circuit that the motor was operated upon. Thus the frequency of the two circuits was the same. By means of a potentiometer scheme *P* any potential difference up to 455 volts (which was the potential across the so-called 440 mains), could be applied to the electrodes of the cell. Connected in series with *A* were a pair of electromagnets, c_1 and c_2. These were placed, one above and the other below, the Braun tube, with their axes in a vertical plane, so as to give a horizontal deflection to the narrow cathode beam which passed through the small hole in the metal screen *s*. The total resistance of the electromagnets was 381.8 ohms. They were fitted with cores of soft Swedish iron, ½ inch in diameter. By means of a double throw switch *S*, the photo-electric cell could be replaced by a non-inductive resistance *R* which was adjusted to allow the same current to go through the electromagnets as passed when the cell was in the circuit. An electrostatic voltmeter *V* gave the fall of potential across the electrodes of the cell.

Method of Taking Oscillograms.

In Plate I., (*a*), (*b*), (*c*), are shown three of the oscillograms taken, These were obtained in the following manner: a photographic film was placed on the drum *d*, Fig. 2, the box closed and a cap put over the lens; the motor was started and adjusted to run in synchronism; the switch *S* was then closed so that the alternating current potential was applied at the electrodes of the cell (the current which passes through the cell also goes through the electromagnets and the alternating magnetic field produced causes an alternating deflection of the cathode beam, so that the spot of light on the fluorescent screen is drawn out into a horizontal

[1] Physical Review, XXVIII., p. 153, 1909.

line); the cap was then removed from the lens and the image of the spot of light moving on the screen was thrown upon the photographic film. The drum was rotated in synchronism and made a complete rotation for every two cycles of the current. So the spot of light moved over the film with two motions, the horizontal motion being produced by the changing magnetic field and the vertical motion produced by the movement of the drum. The spot of light started in at the same point at the beginning of each rotation. The part of the oscillogram obtained by this much of the exposure is the heavy line showing loops only above the horizontal axis, that is, a *current in one direction only*. The switch was then thrown so that R was in the circuit instead of A. The part of the oscillogram due to this exposure is the sine curve. The switch was then opened so that no current went through the electromagnets and consequently there was no horizontal deflection. This exposure gives the line of the axis of abscissas, the axis of zero current. The time of exposure for oscillogram (*b*) for the curve of the rectified current was 2.5 minutes; for the non-rectified current, 1.5 minutes; for the line of the horizontal axis, 40 seconds. So for the total exposure the spot of light moved over the film 8,400 times. This shows that the motor was running in synchronism.

For oscillograms (*a*) and (*b*) the ordinary Eastman kodak films were used. For oscillogram (*c*) an Eastman *extra rapid* film was used. The times of exposures for the three curves of this were 1′ 50″, 50″ and 30″ respectively. Probably 1′, 30″ and 20″ would be plenty of time. The lines are all heavy except in one place, where the film may have been defective or was not evenly developed.

The curve obtained when the current passed through the ohmic resistance R may be called a *potential phase* curve. It is of course a current curve, but since the potential across the ohmic resistance is in phase with the current through that resistance it gives us the phase of the potential about the photo-electric cell, and enables us to determine whether or not there is any lag or advance of the current through the cell. It will be noticed that in every case that the maximum point of the rectified curve is less than that of the potential phase curve. This occurs in spite of the fact that in every case great care was taken to adjust the resistance R before the exposure for an oscillogram so that the maximum deflection of the cathode beam was the same for the current passing through R as it was for the current passing through A. But as the cell is used, that is, as a current passes, the resistance of the cell seems to increase, and consequently the deflection of the cathode beam decreases. This is shown by the broad line of the curve of the rectified current in

oscillograms (*a*) and (*c*) especially. During the exposure for each of these curves the current decreased about 0.5 milliampere, or 10 per cent. This increase of the resistance seems to come about by an absorption of the gas (nitrogen) in the cell. The character of the glow discharge changed in such a way as to show a decrease in pressure. The effect seemed to be much the same as that which occurs with a glow discharge through hydrogen with a potassium cathode in which potassium hydride is formed. But with this difference, that in the cell with hydrogen the potassium hydride formed is of a deep violet color, becoming almost black if the discharge is continued for a long time. But with the cell containing nitrogen the color taken on by the potassium under the action of the glow discharge is at first bronze, and with a continuation of the discharge the color becomes a bright blue, with just a suggestion of a greenish tinge. On heating the cell until the potassium is melted the color disappears, the potassium assumes its original bright metallic luster and the pressure is increased. It seems very probable that a potassium nitride is formed by the glow discharge. Since this work was done a reference[1] has been found giving an account of the formation of potassium nitride by a glow discharge between a potassium cathode and silver anode immersed in a mixture of 90 per cent. liquid nitrogen and 10 per cent. liquid argon; and also the account states that the nitride is formed when the liquid nitrogen is replaced by gaseous nitrogen.

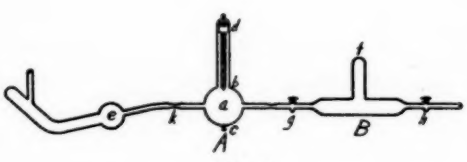


Fig. 1.

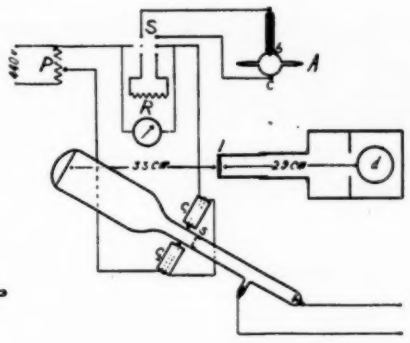


Fig. 2.

In taking all these oscillograms the amount of light falling on the photo-electric cell was rather faint. There was an eight-candle-power red-globed, incandescent lamp about three meters distant from the cell; and the cell was exposed to the faint light from the Braun tube. It was noticed just after oscillogram (*b*) was taken that light falling on the cell had a marked effect on the action. If the cell was in total darkness and

[1] Ber. der deutschen Chemischen Gesellschaft, Vol. 43. p. 1465, 1910.

the potential applied there was no discharge in the tube and no measurable current passed. But if a 16-candle-power lamp at a distance of a meter was turned on the discharge occurred and continued after the light was turned out. But by the deflection of the cathode beam it was noticed that the current was about 6 per cent. less when the cell was in darkness than when it was illuminated by the 16-candle-power lamp at the distance of 1 meter. If however the lamp was brought up to a distance of 40 cm. from the cell, the discharge stopped, but started again when the light was moved away. This shows that the light intensity has a very marked effect, as well as the gas pressure and the electrode distance. It is very

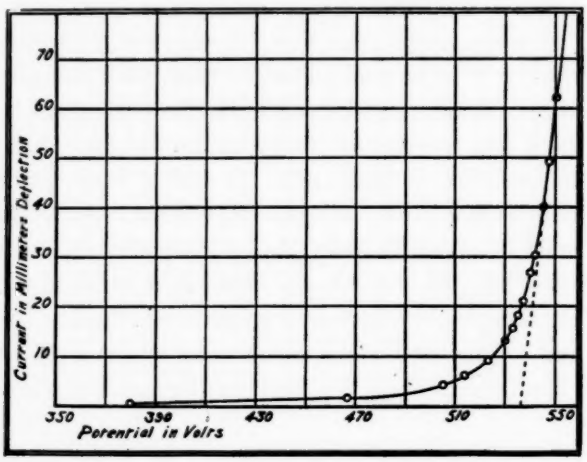


Fig. 3.

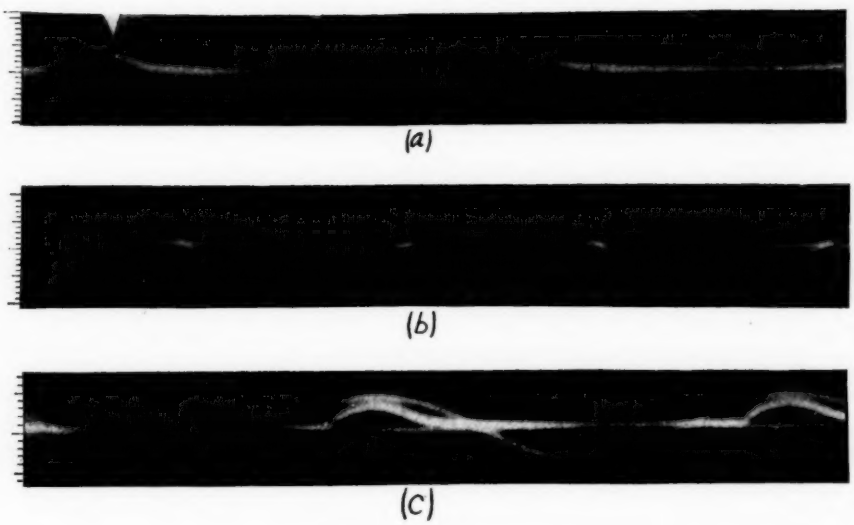
probable that the discharge continued after the light was turned out because of the action of the light produced by the discharge in the cell upon the potassium.

TABLE I.

Oscillogram.	I in Amperes.	V R.M.S. Value in Volts.	Electrode Distance (in Centimeters).
(*a*)	0.004	390	4.1
(*b*)	0.005	455	4.7
(*c*)	0.003	370	4.7

DISCUSSION OF OSCILLOGRAMS.

In Table I. are given the maximum values of the rectified current, I; the R.M.S. voltage across the electrodes, V; and the electrode distance for each oscillogram. The divisions of the scales along the oscillograms represent milliamperes. Time is represented in a horizontal direction from left to right.



S. HERBERT ANDERSON.

The characteristic features of the oscillograms are: (1) That the rectification is complete, that is, the current is transmitted in only one direction; (2) the form of the current curve in the rectified cycle; (*a*) there is a lag in rising from the axis of zero current; (*b*) the curve up to the maximum point is nearly a straight line; (*c*) the curve of decreasing current is of an exponential form; (*d*) the lag at the end of the half cycle is very much less than at the beginning, just how much is difficult to determine because of the exponential form of the curve.

The degree of rectification in this type of cell is shown by another experiment. A cell of the same form and dimensions, but employing potassium hydride as the active electrode and hydrogen for the gas was tried with a direct-current potential. 400 volts had to be applied at the electrodes before a current would pass which could be measured by a milliammeter. With this potential there was a glow discharge in the tube. When electrode *c*, Fig. 1, was connected to the negative terminal of the battery and *b* to the positive, the current was 100 milliamperes. When the terminals were reversed, the current was 0.05 milliampere. The current then passing in one direction was 2,000 times as great as the current in the opposite direction. Such a ratio is much larger than could be shown by the oscillograms.

In general the cause of rectification in this kind of a cell is readily explained. When the potassium electrode is connected to the negative terminal of the source of potential and the platinum point to the positive, electrons are given off readily by a photo-electric action. With such a potential as was used a very faint light is sufficient to start the discharge of electrons. When the field is in the opposite direction no electrons can be given off when the potential difference is greater than that arising from the photo-electric action. However the current due to the electrons alone is of a very much lower value than that obtained here. The author found (l. c.) the electron current from a potassium electrode of about one fourth the area used in this experiment was 6×10^{-10} amperes for the same electrical field. However the light intensity was much greater than in the present investigation. But assuming that the number of electrons leaving unit area of the potassium electrode is the same in the two cases, and hence, that the current due to the electrons in the present investigation is about 24×10^{-10} amperes, even then this is insignificant compared with the total current. So the carriers of the current must be chiefly ions which are produced by collisions of the electrons with the molecules of the gas. As the potential across the electrodes rises from a zero value to a maximum, *c* being negative and *b* positive, the current that can be detected is negligible until the potential difference reaches such a

value that the electrons have sufficient velocity to produce ions by collision. This is indicated by the point where the current curve rises sharply from the axis of zero current in the oscillograms. From this point on as the potential increases to a maximum there is a two-fold increase in the conductivity due (1) to the increase in the rate of production of ions and (2) the increase in the velocity of the ions. As the potential decreases the conductivity decreases in a two-fold manner. Thus we see that the current will not be a sine function because the resistance is not a constant. It is not possible to determine analytically the form of the current curve until the function which shows how the resistance varies is known. When the potential falls below the critical value necessary for the production of ions there are some ions still left in the space between the elecrtodes, and the current from this point on until the potential becomes zero is due to the movement of these ions to the electrodes. When the impressed E.M.F. reverses and c becomes positive and b negative, the electron current is zero, and hence the source of ionization is zero and there is no current.

By the application of the equations for alternating currents we can determine whether the characteristics of the current curve are due to any of the factors of the circuit besides the photo-electric cell. The potential phase curve was obtained when the circuit contained only resistance and the self-inductance of the magnetizing coils. The differential equation of such a circuit with an impressed, simple, harmonic E.M.F. is

$$Ri_1 + L\frac{di_1}{dt} = E \sin \omega t, \tag{1}$$

the solution of which is

$$i_1 = \frac{E}{\sqrt{R^2 + L^2\omega^2}} \sin\left(\omega t - \text{arc tan}\frac{L\omega}{R}\right) + ce^{-\frac{R}{L}t}. \tag{2}$$

When the current has reached a steady state the exponential term becomes negligible. Of the constants of the circuit R and ω are known, but L is not. This may be determined by taking the expression for the maximum value of i_1

$$I_1 = \frac{E}{\sqrt{R^2 + L^2\omega^2}}. \tag{3}$$

The R.M.S. value of the voltage across the electromagnets was 1.5 volts at the time oscillogram (b) was taken. The maximum value of the current, taken from the oscillogram, was 0.005 ampere. The resistance of the coils was 381.8 ohms. The maximum voltage is given by

$$\frac{1.5}{0.707} = 2.12 \text{ volts.}$$

We may then obtain $L\omega$ from (3), where E is the maximum voltage across the coils and R is the resistance.

$$0.005 = \frac{2.12}{\sqrt{(381.8)^2 + L^2\omega^2}},$$

$$L\omega = 184.4.$$

In order to obtain the potential phase curve a resistance of 82,800 ohms had to be introduced into the circuit in place of the cell. The angle of lag of the current in this circuit behind the impressed E.M.F. is given by

$$\text{arc tan } \frac{L\omega}{R}, \tag{4}$$

where R is the total resistance of the circuit and is 82,800 + 382 ohms.

$$\text{arc tan } \frac{L\omega}{R} = \text{arc tan } \frac{184.4}{83,182} = 0.128°.$$

That is, the lag of the so-called potential phase curve behind the impressed E.M.F. is 0.128°. From measurements on oscilligram (*b*) it was determined that the current in the rectified cycle lagged 0.109 part of a period or 39.24° behind the potential phase cycle. Hence the total lag of the rectified cycle behind the impressed E.M.F. is

$$39.24 + 0.128 = 39.368°.$$

We can find the voltage across the electrodes at which the current rises above the axis of zero current by the expression

$$e_0 = E \sin 39.368°,$$

where E is the maximum value of the voltage. The R.M.S. value of the voltage was 455 volts.

$$E = \frac{455}{0.707} = 643.5 \text{ volts.}$$

$$e_0 = 643.5 \sin 39.368° = 408 \text{ volts.}$$

The equation for the current through the cell is given by

$$E \sin \omega t - e_r = R_c i_2 + L \frac{di_2}{dt}, \tag{5}$$

where e_r is the potential drop across the photo-electric cell. Since e_r is a function of the current i_2 the form of this function must be known in order to integrate the above expression. This function is represented by the function potential curve. Unfortunately the author was unable to take such a curve with this cell. In Fig. 3 is shown a current potential curve taken by Dr. J. G. Kemp for a cell filled with hydrogen at 3 mm.

pressure and using an electrode distance of 3.0 cm. Undoubtedly the curve for the cell the author used was of the same type. The analytical expression for this curve has not been determined, but for certain regions of the curve the relation between the current and the potential can be expressed approximately by a linear equation. In the curve, Fig. 3, the maximum value of the current was about 10^{-8} amperes, which is a much lower order of value than the currents obtained in this investigation. Hence it is very probable that the expression for e_r for the range 0.001 to 0.005 ampere is

$$e_r = e_0 + ri, \tag{6}$$

where e_0 is the intercept of the axis of potentials and r is the resistance of the cell. Of course the resistance varies greatly and any value of r that might be determined would only hold for a small range of the current. Substituting the value of e_r given by (6) in (5) we have

$$E \sin \omega t - e_0 = (r + R_c)i_2 + L\frac{di}{dt}. \tag{7}$$

Integrating this equation we get

$$i_2 = \frac{E}{\sqrt{(r+R_c)^2 + L^2\omega^2}} \sin\left(\omega t - \text{arc tan}\frac{L\omega}{r+R_c}\right) + ce^{-\frac{r+R_c}{L}t} - \frac{e_0}{r+R_c}.$$

We may take e_0 to be the voltage that must be placed across the electrodes before a measurable current will pass, that is, 408 volts. In order to determine a value of r, take the maximum value of i_2 from the rectified cycle. Then in equation (6)

$$e_r = E = 643.5 \text{ volts},$$
$$i_2 = I = 0.005 \text{ ampere}.$$

Therefore

$$643.5 = 408 + r \times 0.005,$$
$$r = 46{,}900 \text{ ohms}.$$

This then gives the resistance of the cell at the instant of maximum current.

The exponential term in (8) becomes negligible within a very short time after the circuit is closed and hence should not occur in an equation dealing with current at the time the oscillograms were taken, when a steady state has been reached. It is apparent then that the current curve given by equation (8), when the exponential term is dropped out, will be a sine curve with a lag of arc tan $L\omega/(r + R_c)$ and with the axis of abscissæ raised by an amount $e_0/(r + R_c)$. As previously pointed out the current curve cannot be a sine curve, hence we cannot use this

equation for determining anything about the current except at the instant of maximum value. We can determine then what the lag is at this instant.

$$\text{arc tan}\frac{L\omega}{r+R_c} = \text{arc tan}\frac{184.4}{47{,}282} = 0.224°.$$

This is the largest lag possible arising from the self-inductance of the circuit as r has a minimum value for the maximum value of the current. In as much as this angle is smaller than can be detected on the oscillograms, we may conclude that the lag in the current is due to the properties of the cell as previously noted, and not to other factors of the circuit.

Every electric wave detector of the rectifying type is of use as a detector either (1) because of its rectifying properties, or (2) because its current potential curve is not linear.[1] Inasmuch as the photo-electric cell possesses both of these properties it may be used in two ways, as Dunwoody[2] has shown crystal detectors may be used. In case the rectifying property is made use of the efficiency depends upon the degree of rectification. As in the photo-electric cell the power of rectification is of a very high order, it should prove as efficient as any detector used. It could be used either with a telephone or a sensitive galvanometer. It has the advantage of giving a glow-discharge when a current is passing, so the receipt of a call by wireless could be noted by the operator without the use of the telephone.

The author is now attempting to find the most sensitive condition of the cell for detecting electric waves, and intends to test thoroughly its efficiency.

CONCLUSION.

1. The rectifying power of a photo-electric cell, using potassium for the active electrode, has been found to be of the ratio 2,000 to 1.

2. The general form of the rectified cycle is the same for the different pressures, electrode distances and potentials used.

3. The amount of the current for a given potential depends on the pressure of the gas, the electrode distance, and the intensity of light falling on the cell; but does not increase continuously with increasing intensity.

4. Nitrogen at a pressure of about 5 mm. combines with potassium or is absorbed by it when a glow discharge passes.

5. The high power of rectification and the high resistance of the photo-electric indicate that it may be very efficient as a detector of electric waves.

[1] Fleming, Principles of Wireless Telegraphy and Telephony, p. 474.

[2] U. S. Patent Specifications, No. 837, 616, 1906.

The author takes pleasure in acknowledging his indebtedness to Dr. J. G. Kemp for the type of cell used and for the curve of Fig. 3, to Professor Kunz for many valuable suggestions and assistance in preparing the cells used, and to Professor Carman and the physics department of the University of Illinois, where the investigation was carried on, for the facilities and means for the work.

PHYSICS DEPARTMENT,
UNIVERSITY OF WASHINGTON,
October, 1912.

IONIZATION OF POTASSIUM VAPOR BY ULTRA-VIOLET LIGHT.

By S. Herbert Anderson.

THE general method of determining the ionization of potassium vapor by ultraviolet light consisted in measuring the current produced between two electrodes which were contained in a highly exhausted tube containing potassium vapor, when a given potential difference was applied across the electrodes and a beam of ultraviolet light passed through the vapor between the electrodes.

The form of the tube used is shown in Fig. 1, *A*. The tube was 4 cm.

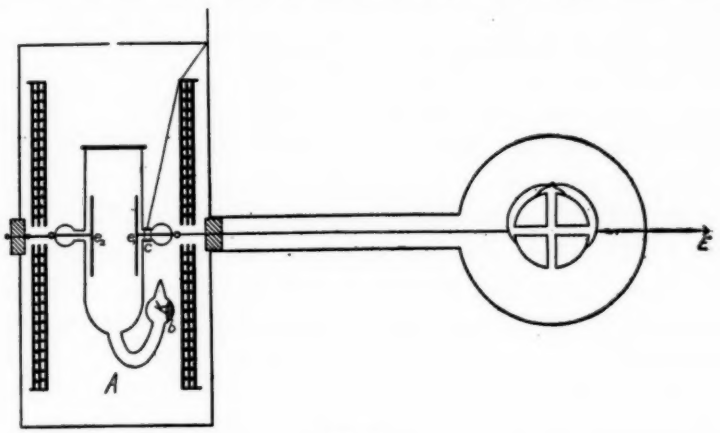


Fig. 1.

in diameter and 12 cm. long. It was closed at one end with a quartz plate sealed on with Bank of England sealing wax. The opposite end of the tube was drawn down to about 8 mm. diameter and terminated in a bulb *b* in which the potassium *k* was lodged. The smaller tube was bent around in a semicircle to prevent particles shot off from the potassium from penetrating directly the space between the electrodes. The potassium introduced into *b* was obtained by distillation and was perfectly pure and clean. The tube was exhausted by a Gaede pump and then by a charcoal bulb and liquid air to the best possible vacuum. The electrodes, e_1 and e_2, were platinum plates 6×2.5 cm., and were placed parallel to each other 3 cm. apart.

In order to test the ionization at temperatures higher than room temperature the tube was placed in an electric furnace consisting of a copper cylinder on which were wound two layers of wire so connected that the magnetic field within was negligible. The electric furnace containing the tube was placed in a sheet iron box, tightly closed except for a slit 5×30 mm. at one end, which could be opened to admit ultraviolet light. This metal box was connected by a metal tube to a tight metal cylinder in which a Dolezalek electrometer was placed. Electrode e_1 was connected to one pair of quadrants of the electrometer. Electrode e_2 could be grounded or connected to one terminal of a battery. c is a metal collar about the tube where electrode e_1 is sealed in. This was grounded so as to prevent conduction over the outside of the tube from effecting the electrometer. The copper cylinder of the heating coil and the metal containers of the heating coil and electrometer was grounded, E.

When the tube was kept in darkness at room temperature, 25° C., no current could be detected with the electrometer when a potential difference of 1,000 volts was maintained between the electrodes. When a beam of ultraviolet light from a spark between zinc electrodes was passed into the tube, but not striking the electrodes, and the same potential difference, 1,000 volts, was maintained, e_2 being connected to the positive terminal of the battery, there was still no current. (The electrometer was capable of indicating a current of 10^{-13} amperes.) If however the beam of light was incident upon the electrode connected to the electrometer, there was a deflection and the direction of the current was from e_2 to e_1. This of course was due to the photo-electric action of the platinum electrode. Hence there was no indication of ionization at a temperature of 25°.

The tube was then heated to 55°, which is the highest temperature to which the sealing wax may be heated without softening. Two hours was required for this heating, in order to get a constant temperature. At this temperature when a beam of ultraviolet light was passed between the electrodes there was a comparatively large current, the values of which for different potential differences are given in column (a) of the table. It was found that these readings could not be duplicated but that the current increased as the tube was maintained at this temperature. After four hours another set of readings was taken which is given in column (b). The tube was then allowed to cool down to room temperature and the conductivity tested when a beam of ultraviolet light passed between the electrodes. The current was found to be about the same as that found by the last set of readings at 55°, as is seen by comparing column (c) with column (b) (Fig. 2).

The tube was in perfect condition after the heating. The vacuum was tested by a spark discharge and found to be at the Röntgen ray stage. The discharge showed some of the characteristic color due to potassium vapor.

In the table the current is given in terms of the rate of deflection of the electrometer needle in mm. per second. A deflection of 20 mm. per second corresponds to a current of 10^{-12} amperes. The current observed is surprisingly large if one takes into consideration the fact that at 55° the vapor pressure of potassium is not large. The minimum current observed

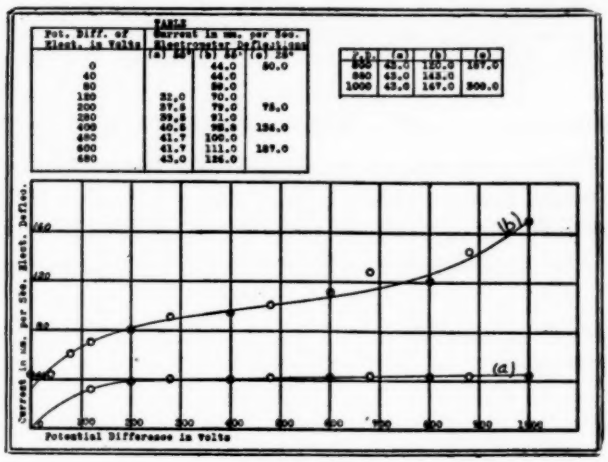


Fig. 2.

was 1.5×10^{-12} ampere, that is, an order of magnitude quite different from the currents due to ionization of any other vapor or gas by ultra-violet light.

The increase of conductivity as the tube was maintained at a temperature of 55° is not easy to explain; nor the fact, that when the tube had cooled to room temperature the conductivity was about the same as that last observed at 55°, while in the first place there was no current at room temperature. However, it is likely that this is closely connected with the phenomenon observed by Wood[1] in the resonance spectra of mercury vapor. He found that there was a true absorption of light only when the pressure of the gas was above 0.01 mm. When a small amount of air was introduced so that the pressure was raised above 0.01 mm. the absorption increased with the pressure. It is not unreasonable to expect that ionization accompanies the transformation of light energy into heat energy that occurs in true absorption. If this is the case an introduction

[1] Phil. Mag., V., 23, p. 689, 1912.

of a small amount of air or other gas into the tube of this experiment would result in an increase of conductivity. In this experiment it is very probable that gas was given off from the electrodes and walls of the tube as the temperature was maintained at 55°. On the basis of Wood's discovery this then would account for the increase in conductivity. No deposit of potassium was observed in the tube, after the temperature had been raised to 55°. Moreover, if there had been a deposit of metal, it would have occurred during the distillation, as the temperature was then above 200°, and yet no conductivity or ionization was noticeable until the tube was heated to 55°. It is the intention of the author to extend this investigation and determine this point.

SUMMARY.

1. Potassium vapor at 55° is readily ionized by ultraviolet light.

2. An increase of ionization is produced by the addition of a small amount of foreign gas.

The writer is indebted to the Laboratory of Physics of the University of Illinois for the facilities and to Dr. Kunz for suggestions for the above work.

UNIVERSITY OF WASHINGTON,
November, 1912.

PROCEEDINGS

OF THE

AMERICAN PHYSICAL SOCIETY.

A METHOD OF PRODUCTION OF LIGHT-NEGATIVE SELENIUM.[1]

BY F. C. BROWN.

IN the previous work[2] having to do with that variety of selenium that decreases its conductivity with illumination, no definite instructions have been given as to a reliable method of production. In fact no reliable method was known. Recently I have found two methods for producing this light-negative variety which are successful and apparently easy to follow. Both depend on the action of mercury vapor on selenium. The first method requires only that the vitreous or amorphous selenium be crystallized at temperatures anywhere between 170° C. and 200° C., over a sand bath in which there is a trace of mercury. Thirty minutes is usually sufficient time for this crystallization.

The second method has been to place a sample of light-positive selenium such as found in the Giltay cell in a vacuum created by a mercury pump. After several hours in such a vacuum the mercury vapor causes the resistance to fall from upwards of a million ohms to a value as low as 20 ohms without changing from room termperatures. When this lower resistance is reached the selenium becomes light-negative.

It is believed that the mercury vapor is directly responsible for calling forth the light-negative property. This may occur by one of three processes. The mercury may form conducting films through and around the selenium. Accordingly these films are broken by the changing volume and form of the selenium crystals under the action of light. This would require a mercury film of average thickness less than a millionth of a centimeter. Secondly the mercury may act as a catalyzer, thereby merely altering the rates of change between components of the selenium. And another explanation might be on the basis of the formation of a selenide by the mercury. The plausibility of the explanations is ranked in the order in which they are above stated.

[1] Abstract of a paper presented at the Evanston meeting of the Physical Society, Nov. 30, 1912.

[2] Paper by F. C. Brown, PHYS. REV., 33, p. 1, and Phys. Zeits., 11, p. 482, 1911.

[3] Paper by Lilah B. Crum, PHYS. REV., 33, p. 538.

SUMMARY OF A STUDY OF THE ELECTRIC FURNACE SPECTRUM OF IRON.[1]

BY ARTHUR S. KING.

THE iron spectrum for the range from λ 2500 to λ 7200 as given by the tube resistance furnace in the Pasadena laboratory has been studied at temperatures ranging from 1800° to 2600° C. These temperatures were read by a Wanner pyrometer directed at the interior wall of the heated tube. The photographs of the visible spectrum have been made on large scale plates and at temperatures designated as "low," "medium," and "high," for which the measurements were about 1800°, 2100°, and 2500° C. respectively. Estimates of the intensities of lines in the arc were made in addition to those for the three furnace temperatures. From these intensities, it was possible to form a classification of the lines according to the temperature at which they first appear, and their rate of growth with increase of temperature, one of the main points brought out in the investigation being that some lines strengthen much faster than others as the temperature rises.

Six classes of lines are formed on this basis. Of these, the first two, designated as Classes I *A* and I *B*, appear at the lowest temperature and are strong in the furnace at all temperatures. The peculiarity of Class I *A* is that these lines are very weak in the arc spectrum. The lines of Class II appear also at low temperature but strengthen much more rapidly than the preceding classes as the temperature rises. Class III lines first appear distinctly at about 2100° and show a rapid increase of intensity at higher temperature and in the arc. Lines of Class IV appear only at the highest furnace temperature, while those of Class V are faint or absent in the furnace, though frequently strong in the arc.

The furnace at 2600° gives a spectrum in which a large percentage of the arc lines appear, but the relative intensities are very different from those of the arc spectrum. However, except for the lines of Class I *A*, for which the arc conditions are decidedly unfavorable, the intensities shown in the arc are of the order to be expected from the rate of strengthening with rising temperature in the furnace, if the arc discharge is considered as giving an excitation equivalent to a much higher temperature than is attainable in the furnace, where an upper limit is set by the strong vaporization of the carbon tube. If the melting and boiling points of iron are taken as 1500° C. and 2450° C. respectively, it is evident that the vapor can be made to radiate much below its boiling point. Since the highest furnace temperature is above the boiling point, the arc should give a spectrum similar to that of the furnace if the iron were simply boiling in the arc. It would thus seem that the differences between arc and furnace line intensities are to be attributed mainly to the high electronic speeds given by the arc discharge.

The changes in intensities of lines from the core to the outer vapors of the

[1] Abstract of a paper presented at the Evanston meeting of the Physical Society, Nov. 30, 1912.

iron arc were examined by projecting an image of the horizontal cross-section of the arc on the slit of the plane-grating spectrograph. The low temperature furnace lines were found to extend farthest into the outer vapors of the arc, the high temperature lines being giving chiefly by the core. The increase in strength from the flame into the core was also found to be similar to the rate at which the lines strengthened with increasing furnace temperature. The results thus indicate an equivalence between the excitation in the different parts of the arc and that which would result from temperature differences. Chemical processes in the flame of the arc do not appear to be an active agent in the phenomena, since the furnace *in vacuo* at low temperature, which should give minimum chemical action, gives a spectrum most nearly resembling that of the outer vapors of the arc.

The furnace material offers many instances where two lines near together show quite different response to temperature change and their relative intensities in a given source of light may be used to indicate its temperature when referred to the furnace spectrum for a known temperature. This method is applied to the flame spectra of iron obtained by Hemsalech and de Watteville for various gas mixtures and the several flames are arranged in order of their effective temperatures in producing radiation from iron vapor.

The ultraviolet furnace spectrum was found to be very rich in lines. As would be expected, the spectrum extended farther toward shorter wave-length as the temperature increased. It was found, however, that the line spectrum did not extend as far as the continuous spectrum given by a plug in the middle of the tube for the same temperature. Aside from this, the distribution of lines through the spectrum bears little resemblance to the intensity gradation shown by the spectrum of an incandescent solid. The low temperature lines are chiefly in the blue and green, higher temperatures being required to give lines in the orange and red.

The furnace experiments do not offer definite evidence as to the possibility of producing a line spectrum by temperature alone, since there is strong ionization from the carbon tube and a limited amount of chemical action. However, the position of temperature as the agent which gives the initial excitation and controls the observed variations in the spectrum seems to be clear. A potential difference is not necessary to the existence of the line spectrum. The spectrum fades gradually when the current is broken, some iron lines having been observed in the heated tube 5 minutes and the *D* lines of sodium 16 minutes afterward. A plug in the tube serves to suppress or throw into absorption all the lines of a vapor also present, indicating that the black body radiation is stronger than that of a metallic vapor for any given wave-length.

MOUNT WILSON SOLAR OBSERVATORY,
November 7, 1912.

THE EARTH INDUCTOR AS AN INCLINOMETER.[1]

BY N. E. DORSEY.

THE instrument chiefly used for the determination of the magnetic inclination is the dip needle. Although very simple in theory, this instrument in practice is subject to many well known imperfections, the most serious of which are those known as pivot errors. These errors vary with the inclination, and can not be eliminated by any combination of observations. For this reason another universal instrument for the determination of the dip is imperatively needed.

The most promising of such instruments is the continuously rotating flat coil earth inductor, when used with a suitable galvanometer and a two-part commutator. In the past the theory and the use of this instrument has been limited to the simplest possible case, viz., that in which the axis of rotation lies in the magnetic meridian and at such an inclination that the deflection of the galvanometer is zero. Then, except for the effect of thermal electromotive forces, the axis of rotation lies in the direction of the earth's field. The thermal current is eliminated by a suitable combination of observations taken with the coil rotating in opposite directions.

This procedure is very good when the meridian can be readily determined and when the instrument has a firm support, but it breaks down when observations are desired near the poles or at sea. In the first case the meridian can not be readily determined with the requisite accuracy, and in the second the instrument is never at rest. In neither case, however, would there be serious difficulty in the determination of the dip had we but a perfect dip needle. When oscillating, as on ship board, the mean of a sufficient number of turning points would be its correct position; and when at rest the sum of the squares of the cotangents of the inclinations of the needle at two azimuths differing by 90° equals the square of the cotangent of the true dip.

A consideration of the theory of the earth inductor shows that when the brushes are so adjusted that commutation occurs when the mean plane of the coil is parallel to the inclination axis of the instrument, then that inclination of the axis of rotation of the coil for which the deflection of the galvanometer, in both magnitude and sign, is independent of the direction of rotation of the coil (the speed being the same for both directions) is exactly that which would be taken by a perfect dip needle when its pivots are parallel to the inclination axis of the inductor. Furthermore, when the instrument is oscillating, that inclination for which the mean of the turning points of the galvanometer is independent of the direction of rotation of the coil, speed constant, is equal to the mean of the turning points of a perfect dip needle having its pivots parallel to the inclination axis of the inductor.

Consequently, under all conditions the earth inductor can be used in such a manner as to give the same indications as would be given by a perfect dip needle.

[1] Abstract of a paper presented at the Cleveland meeting of the Physical Society, December 30, 1912.

The above assumes that the thermal electromotive force is independent of the direction of rotation, the speed being constant; or that it has been eliminated by suitable reversals of the coil terminals with reference to the commutator segments.

Since there is comparatively little wear upon the brushes it is probable that these will require resetting only at relatively long intervals. They can be set wherever the magnetic meridian is known. To do so, place the inclination axis in the magnetic meridian, and the axis of rotation of the coil vertical; then the brushes will be properly set when the deflection of the galvanometer is independent of the direction of rotation of the coil.

According to advices received by cable from Chile, an earth inductor now in use upon the *Carnegie* is giving perfect satisfaction. The instrument has not yet been tried out in the neighborhood of the magnetic poles.

DEPARTMENT OF TERRESTRIAL MAGNETISM,
CARNEGIE INSTITUTION OF WASHINGTON.
(To be published in the Journal of Terrestrial Magnetism for March, 1913.)

THE USE OF SOUNDING BOARDS IN AN AUDITORIUM.[1]

BY F. R. WATSON.

SOUNDING boards have long been used as an aid to speaking in auditoriums, especially in churches. The author has tried the effect of sounding boards of different kinds in the auditorium at the University of Illinois, where the acoustical properties are unsatisfactory. A plane reflector placed at an angle over the speaker's position proved to be of little benefit. The same was true for a sheet of heavy canvas. A pronounced effect was found when a parabolic reflector was used. The main volume of the sound was directed as desired and a number of troublesome echoes eliminated. The results indicate that the use of a reflector designed for the dimensions and shape of an auditorium will be an aid to good hearing. Experiments are now being conducted in which reflectors of special design are being made to direct the sound more effectively to the audience.

THE EFFECT OF MAGNETIZATION ON THERMAL CONDUCTIVITY.[1]

BY N. F. SMITH.

CONTRADICTORY results have been obtained by other observers. The present investigation has been confined, thus far, to a determination of the change produced in the thermal conductivity of iron by a longitudinal field. Observations have been made on metal tubes three eighths of an inch in diameter and about three feet in total length. The iron tube and a comparison tube of brass were mounted side by side. One end of each of these tubes was maintained at a constant temperature of from 200° to 225°. The two tubes were surrounded by similar solenoids, connected in series. The coil surrounding the comparison tube was wound non-inductively, while the winding of the

[1] Abstract of a paper presented at the Cleveland meeting of the Physical Society, December 30, 1912.

other could be changed from non-inductive to inductive by a commutator. By a suitable arrangement of thermo-couples, two points of equal temperature were determined in the two tubes while a non-inductive current flowed in each coil. The magnetic field was then applied to the iron tube by shifting the commutator, and after time had been given for temperature conditions again to become steady, the point of equal temperature in this tube was again determined. Fields up to 163 C.G.S. units were employed. The sensitiveness of the apparatus was such that a change of conductivity of one tenth of one per cent. could have been detected with certainty, yet in no case was an effect as great as this observed.

CENTRE COLLEGE,
DANVILLE, KY.

LINE STRUCTURE AND SHIFT WITH AN ECHELON SPECTROSCOPE.[1]

BY NORTON A. KENT.

IN an article,[2] dealing with line structure and shift as revealed by an echelon spectroscope, the writer showed that the inequality of wave-length between certain arc and spark lines in the spectrum of zinc could be explained by the appearance of satellites on the red side of the main lines. The results obtained confirmed those of previous investigations with large Rowland gratings and showed how the satellite systems varied with the capacity and inductance in the circuit and were modified by using an alloy for the spark terminals and by throwing upon the slit of the spectroscope various regions of the spark.

The writer's attention has since been called to a paper by Stansfield,[3] and the suggestion made that possibly the satellites obtained were those due to a secondary action of the echelon. The writer after further experimentation concludes that this cannot be the case, for the following reasons:

1. "The secondary bands are superposed on the echelon lines and resemble them in appearance. Their width is about the same as that of the finer spectrum lines and so in the ordinary position of the echelon they are not easily recognized."[4] The satellites observed are by no means "fine," but are as diffuse as the main line; moreover, they are "easily recognized."

2. "When the echelon is rotated the secondary bands move faster than the spectrum lines in the same direction and so move across them. No such phenomenon has been observed in the case of the satellites."

3. The secondary bands are "curved in the same direction" as the spectrum lines "but more strongly." The satellites observed are of the same curvature as the main lines.

4. The secondary bands should be about one seventh the distance between the adjacent orders, as given by the echelon. The satellites are usually sepa-

[1] Abstract of a paper presented at the Clevcland meeting of the Physical Society, December 30, 1912.

[2] Proceedings of the American Academy of Arts and Sciences, Vol. XLVIII., No. 5, August, 1912.

[3] Philosophical Magazine (6), 18, 383, 1909.

[4] The quotations are from Stansfield's paper.

rated from each other and the main line about one third or one fourth the distance between the orders.

5. The structure is shown by two echelons made by different manufacturers.

Moreover, it may be added that the satellites are not due to false structure owing to the errors of the instrument. Their reality has been tested by the Koch method.[1]

PHYSICS LABORATORY,
BOSTON UNIVERSITY.

VACUUM TUBE DISCHARGE IN A MAGNETIC FIELD.[2]

BY NORTON A. KENT AND ROYAL M. FRYE.

ALTHOUGH many have studied vacuum tube discharge in a magnetic field, no investigator has fully identified the resulting spectrum and classified the effects obtained upon the creation of the field.

The writers have found that the decrease of the effective cross-section of the tube, and the sand-blast action of the ions deflected against the glass—both caused by the magnetic field—result in the production of one or more of the following classes of spectra:

1. Those of impurities in the tube; examples, hydrogen appearing in an argon tube, and argon in a nitrogen tube.

2. Those of dissociation products of the original gas; example, the oxygen spectrum appearing in a carbon monoxide tube.

3. Those of dissociation products of the glass; example, sodium and oxygen lines appearing in the spectra given by many tubes when in the field.

The case of nitrogen and argon above mentioned is of special interest, as some of the lines of the blue spectrum of argon persist for at least ten minutes after the magnetic field is removed, and the reappearance of the band spectrum of nitrogen is but gradual. This is a unique phenomenon.

PHYSICS LABORATORY,
BOSTON UNIVERSITY.

RESISTANCE OF OXIDES AT HIGH TEMPERATURES.[2]

BY A. A. SOMERVILLE.

THE resistance of ten oxides is determined for a temperature range between room temperature and higher to about 1000 degrees Centigrade. In some cases the oxides are almost perfect insulators at room temperature. In practically all cases the resistance decreases with rise of temperature.

Cadmium oxide is one exception to the above rule. It is a fairly good conductor at ordinary temperatures. The resistance decreases slowly for a time when heated, and upon continued rise in temperature the resistance rises also, only to decrease again and at 1100 degrees is practically the same as at 20 degrees.

Silicon monoxide is a new product and the lightest of any oxide, its density being .02 grams per cubic centimeter.

[1] P. P. Koch, Ann. der Phys., 34, p. 386, 1911.

[2] Abstract of a paper presented at the Cleveland meeting of the Physical Society, December 30, 1912.

A MICROPYROMETER.[1]

BY GEORGE K. BURGESS.

A SMALL incandescent lamp is mounted within the Huyghens eyepiece of an ordinary microscope which is then used as a Morse or Holborn-Kurlbaum pyrometer. Combined with a suitable furnace, such as a platinum or other metallic strip heated electrically in vacuo or suitable gas as hydrogen, melting points and emissivities of minute specimens (0.01 to 0.001 milligram or less) are readily observed. The apparatus is most conveniently calibrated empirically in terms of known melting points. It is being used for measurements on the rare refractory elements and the author will be glad to receive pure samples of a few hundredths milligram, particularly of the rarer elements, for the determination of their melting points and emissivities.

December 12, 1912.

OPTIMUM WAVE-LENGTH IN RADIOTELEGRAPHY.[1]

BY A. H. TAYLOR.

GIVEN a certain available power, furnished by a self-controlling transformer, and a fixed aerial system, the determination of the optimum wave-length for the station resolves itself into the determination experimentally of the maximum attainable value for the expression $I^2/\lambda^2 P$ which represents the radiant efficiency of the station, and of the maximum value of I/λ, which is proportional to the maximum sending distance in an ideal non-absorbing medium. The first of these quantities is a function of λ but not of P; the second is proportional to $\sqrt{P}$, where I = sending current, λ = wave-length, and P = power input.

The first of these expressions, $I^2/\lambda^2 P$, is proportional to the audibility at close range. The second, I/λ, has its effect modified by the absorption term, $\epsilon^{-4a/\sqrt{\lambda}}$.

Using data obtained by Austin[2] the effect of absorption for over sea transmission can be calculated. This effect depends on the available power and on the height of the aerials, but it seems clear that for low power installations the shortest wave-length consistent with good coupling will give the best results.

At distances of 300 to 600 kilometers, the calculations indicate that a very considerable variation in wave-length will affect the range but little. The short wave-lengths should be much more effective at close range. These conclusions have been partly verified by experiment.

The following variations from the deductions may be predicted:

First. Increase of power will push the optimum wave-length toward longer waves.

Second. Increase of height of sender or receiver will make the optimum wave-length longer.

[1] Abstract of a paper presented at the Cleveland meeting of the Physical Society, December 30, 1912.

[2] L. W. Austin, Bull. Bur. St., Vol. 7, No. 3, 1911.

Third. Inefficient aerials (K smaller) will make the optimum wave-length shorter.

Fourth. Increased absorption will make the optimum wave-length longer.

Fifth. Increased radiation efficiency $i^2/\lambda^2 P$, as by synchronous rotary spark gap, impact excitation, etc., will tend toward a longer optimum wave-length.

THE SIMILARITY OF ELECTRICAL PROPERTIES IN LIGHT-POSITIVE SELENIUM TO THOSE IN CERTAIN CRYSTAL CONTACTS.

BY T. C. BROWN.

SINCE so many of the electrical properties of selenium have been supposed to be unique, it is thought to be significant that most of these properties also appear in connection with crystal contacts. The like phenomena that are compared are:

1. The apparent invalidity of Ohm's law.
2. The variation of the conductivity with pressure.
3. The change of the resistance with time of current action.
4. The effect of alternating currents.
5. The decrease of resistance by slight amalgamation.
6. The effect of abrasion.

Attention is further called to the fact that the light effect could be explained by merely assuming a change in crystal volume or crystal form as the result of light-action. This light effect is the only important electrical phenomenon that is known not to have its counterpart in crystal contacts.

COMPARISON OF SMALL ELECTROSTATIC CAPACITIES.[1]

BY J. C. HUBBARD AND H. F. STIMSON.

THE electrostatic capacities of small bodies such as lead wires, binding posts, electrometers, small spheres, or the change in capacity when two such bodies are joined, as when a lead wire is connected to a sphere, are compared by the method of divided charge with the capacity of a straight wire used as a standard. The instrument making these experiments possible is the gold-leaf electroscope of constant capacity described by one of the authors.[2]

It is first necessary to measure the capacity of the electroscope and its lead wire. The latter was chosen about 1 meter long. About 20 cm. of its free end is made to move through a small angle. In its lowest position this wire touches an earth connection and the comparison capacity (a length of bare wire stretched between two pieces of red sealing wax) thus putting the gold leaf and standard at zero potential. Raising the lead wire breaks, first the earth connection, then connection with the standard. Further raising puts the lead wire in contact with a terminal of a suitable battery (ten volts) which charges the gold leaf. Lowering the lead wire brings it in contact with the standard, with which the charge is thereby divided, and still further lowering earths

[1] Abstract of a paper presented at the New York meeting of the Physical Society, October 12, 1912.

[2] PHYS. REV., 33, p. 558, December, 1911.

both the electroscope and wire standard capacity again. In the meantime potential readings for these various positions are taken with the electroscope. The measurements are complicated by the fact that the movement of the electroscope lead wire slightly varies the capacity both of the electroscope system and of the standard. For example, the combined capacity of the two is less in some cases by more than 1 cm. when they are in contact than when they are separated. But it is only when the electroscope lead is within a fraction of a cm. in distance from the standard wire that its influence must be taken into account. In fact one of the most interesting results of these measurements it the fact that wire standards by reason of the considerable extension made possible, are not seriously affected by the movement in their neighborhood of comparatively small bodies charged to low potentials. It was not found necessary to make any correction for change in capacity of the electroscope system when the lead was withdrawn from the charging battery terminal, as the change was well within the errors of observation.

As an example of the accuracy obtainable there is here given the result of comparing the capacity of the electroscope with the capacities of three wires of different lengths. Let V_1 be the potential of the charged gold leaf before the charge is divided; V_2, the potential of the divided charge; E, the capacity of the electroscope and lead; and C, the capacity of the comparison wire. Then

$$EV_1 = (E + C - d)V_2,$$

where d is the amount by which the capacity of the combined system is diminished on account of the contact of the electroscope lead with the standard. Let l be the length of the standard wire and a, the radius. Instead of using V in volts it is more convenient to express it in terms of electroscope scale readings, here proportional to the potential. All of the wires had a radius of .0253 cm. From the values given in the table, d is found to be 1.51 cm. of capacity, the

l cm.	C cm.	V_1	V_2	E cm.
370.6	19.32	459	160	9.53
269.8	14.55	464	195	9.44
135.5	7.89	460	275	9.48

values of E being respectively 9.53, 9.44, and 9.48 cm., giving a mean of 9.48 cm. It is of interest to note that of this capacity of 9.48 cm., about 6 cm. is due to the lead to the electroscope, leaving about 3.5 cm. as the capacity of the gold leaf and its connection.

The capacity of a circular plate was measured in the following manner. Two equal straight wires were attached perpendicularly to the center of the disc, one on each side. The capacity of the whole system was then measured, using one of the wires as the lead. The other wire was then removed and the capacity measured again. The first value minus the second gives the amount by which the capacity is changed by the presence of one lead. Subtracting this amount from the second value should give the capacity of the disc alone.

Thus, with two leads, each two meters long and of a diameter of .1025 cm., and a plate having a diameter of 30.5 cm., the combined capacity was found to be 27.5 cm. Removal of one lead gave 18.5 cm., being less by 9.0 cm. Subtracting this amount from 18.5 cm. leaves 9.5 cm. as the measured capacity of the plate. The capacity as calculated from $C = 2r/\pi$ is 9.7 cm.

One condition very essential to the success of the measurements is that there shall be no alternating current potentials within several meters of any part of the apparatus. When the alternating current was not cut out of the room in which the experiments were carried on the electroscope gave erratic deflections of large magnitude every time connection between it and any other body was broken.

The method here outlined has been used for measuring the capacities of the lead wires, levers, and connections in the oscillation experiments described in the next abstract. It may equally well be used for the measurement of the capacity of bodies used in ionization experiments.

THE DETERMINATION OF THE PERIOD OF ELECTRICAL OSCILLATIONS.[1]

BY J. C. HUBBARD.

THE method consists in starting a train of damped oscillations and in determining the times between successive nodes. Let the inductance L (see Fig. 1) and the capacity K, be connected in parallel, and let a battery, B, be connected in series with the inductance and a resistance R. A lever, 1, is in the circuit and, when struck by a falling weight, breaks the circuit and starts a series of damped oscillations. The drop chronograph described by Professor Webster[2] has been used as the circuit breaker. The conditions for breaking the circuit so as to have no spark at the break have been worked out by the author.[3] A carriage moved vertically by a micrometer screw carries levers 2 and 3 and their stops shown in the figure. The falling weight, after striking 1, descends through a measured distance and strikes 2. Lever 2 turns on its axis in the center and lifts lever 3 from the screw which is connected to the quadrant electrometer. The switch at a was opened in advance, leaving the quadrants of the electrometer free and at zero potential. It will be seen that connection has been established between the electrometer and the oscillating system *only* during the instant that lever 2 was lifting lever 3 from the supporting screw, for no sooner has lever 3 moved than connection with the electrometer has been broken. Thus, the latter has been connected across the

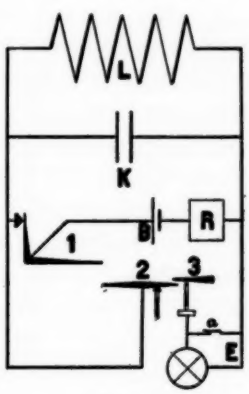


Fig. 1.

[1] Abstract of a paper presented at the New York meeting of the Physical Society, October 12, 1912.

[2] A. G. Webster, PHYS. REV., VI., pp. 297–314, 1898.

[3] J. C. Hubbard, PHYS. REV., XXII., pp. 129–158, 1906.

condenser terminals for a very brief instant, and at a time after the starting of the train of oscillations which is controlled by the vertical height of lever 1 above lever 2. If the electrometer reading is zero, connection has been made at a node of potential. The method of procedure consists in finding the first five nodes, then in calculating approximately and ascertaining exactly by trial the positions of the 10th, 15th, 20th, 25th, 50th, and 100th. Or, if the period is very large, sufficient accuracy may be reached with the determination of as few as five consecutive nodes. The height of fall of the projectile may vary from a few cm. to 100 cm. depending upon the frequency to be measured. The greater the height the greater the distance between successive nodes and the greater the accuracy with which they are determined.

Experiments have been carried out during the past summer with the assistance of Mr. H. F. Stimson. Two coils have been experimented upon. One is a coil of 60 layers of wire with 60 turns in each layer. This coil has been quite fully described by J. E. Ives.[1] The other is the single layer standard of inductance wound on a marble cylinder and described by J. G. Coffin.[2] The capacity used is the standard condenser described in the paper of Professor Webster already referred to. The capacity of leads, levers, and connections was measured by the method described in the preceding abstract. Values of capacity used varied from 200 to 1,500 cm., and of inductance, from .018 to 3 henries. The results obtained with the Ives coil show the large distributed capacity in coils of more than one layer. When only ten layers of the coil were used the distributed capacity was several hundred cm., and increased rapidly with the frequency. The effect of the increase is to make the actual period in some cases twice as large as that calculated from the inductance and the known external capacity. Much more simple results were obtained with the marble standard of inductance. A single set of observations is here given for the purpose of illustrating some of the magnitudes involved.

Let $K(obs)$ be the total external capacity including the capacity of the standard and connections, *i. e.*, all but the distributed capacity of the coil. Let $K(calc)$ be the capacity calculated from the formula $T = 2\pi\sqrt{LK}$, where $L = .01822$ henry. The variation of L with the frequency is neglected for the present. The inductance consisted of the double strand marked B on the coil, the two wires being in parallel. The height of fall, h, of the projectile to lever 1 was 53 cm. Let s be the distance passed through by the projectile after striking lever 1 while 100 cycles take place. Then

$$T = \frac{1}{100}\sqrt{\frac{2h}{g}} \cdot \{\sqrt{1 + s/h} - 1\}.$$

The difference between observed and calculated values of the capacity is doubtless mostly due to the distributed capacity of the coil, though the variation of the inductance with the frequency is not without effect. The coil is not

[1] J. E. Ives, PHYS. REV., XIV., p. 298, 1902.

[2] J. G. Coffin, Bull. Bureau of Standards, II., pp. 87–143. 1906.

TABLE.

s, cm.	$T \times 10^5$.	K (calc.) cm.	K (obs.) cm.	Diff.
.8815	2.723	927.5	845.3	82.2
.7460	2.305	664.8	584.4	80.4
.6650	2.055	528.5	446.9	81.6
.6015	1.861	433.4	351.1	82.3
.5155	1.595	318.2	230.3	87.9

satisfactory for use in these experiments on account of the double winding which makes it impossible to apply known formulæ for either inductance or distributed capacity under the conditions necessary in these experiments. Suitable single layer coils are being designed for use in a systematic study of the effect of varying frequency upon the inductance and the distributed capacity. It may then be possible to get some quantitative data of value with coils of more than one layer of turns.

THE CONSTANTS OF SPECTRAL RADIATION OF A UNIFORMLY HEATED ENCLOSURE OR SO-CALLED BLACK BODY.[1]

BY W. W. COBLENTZ.

IN previous communications to this society accounts were given of the progress made in the investigation of the constants involved in the formulas, proposed by Wien and by Planck, for expressing the partition of energy in the spectrum of a so-called black body.

In view of the fact that the great mass of theoretical speculations seemed entirely out of proportion to the slender experimental data upon which they were based, it seemed desirable to obtain an extensive series of observations under all sorts of conditions, leaving the exact computation of the results until the very last, and then computing all the data on a uniform basis. From the beginning of the work, four years ago, attention was called to the fact that the Wien equation does not fit the observed spectral energy curves, and in later communications, the computations were made on the basis of the Planck equation, introducing all the known correction factors which can effect the observed spectral energy curves. These factors are (1) corrections for the selective reflection of the silver mirrors, the fluorite prism, and the fluorite window which covers the vacuum bolometer; and (2) the corrections for the variation in reflecting power with angle of incidence upon the silver mirrors and upon the fluorite prism.

In the isothermal spectral energy curves the position, λ_{max}, of the maximum emission, E_{max}, is computed by taking the wave-lengths, λ_1 and λ_2 corresponding to equal emissivities, $E_1 = E_2$, on the assumption that the observed energy curve fits the Planck equation:

$$E_\lambda = c_1 \lambda^{-5} (e^{c_2/\lambda T} - 1)^{-1},$$

[1] Abstract of a paper presented at the Cleveland meeting of the Physical Society, December 30, 1912.

from which follows

$$(2)\quad \lambda_{max} = \frac{\alpha(\log \lambda_2 - \log \lambda_1)\lambda_1\lambda_2}{\alpha'(\lambda_2 - \lambda_1)\log e} - \frac{\lambda_1\lambda_2[\log (1 - e^{-c_2/\lambda_1 T}) - \log (1 - e^{-c_2/\lambda_2 T})]}{\alpha'(\lambda_2 - \lambda_1)\log e}.$$

The second term in this equation can usually be abbreviated, since terms involving λ_1 are usually negligible. For values of λ_2 which are less than about 4μ the term $\log(1 - e^{-c_2/\lambda_2 T})$ may be expanded into a series and (by dropping all terms but the first) may be used in the form $-e^{-c_2/\lambda_2 T}\cdot\log \epsilon$.

In this equation $\alpha' = 4.9651$ and $c_2 = \alpha'\lambda_m T$. For computing the second term correction factors to λ_m, the value of $c_2 = 14{,}500$ was used. However a variation of 100 units in c_2 (*e. g.*, $c_2 = 14{,}600$) would change the mean value by only 0.0005μ, which is negligible.

For computing the constant, c_2, from an isochromatic energy curve, at any wave-length, λ, Planck's equation is used in the following form:

$$(3)\quad C_2 = \frac{(\log E_2 - \log E_1)\lambda T_1 T_2}{\log \epsilon(T_2 - T_1)} - \frac{(e^{-c_2/\lambda T_2} - e^{-c_2/\lambda T_1})\lambda T_1 T_2}{T_2 - T_1}$$

where E_1 and E_2 refer to the emissivities corresponding to the temperatures T_1 and T_2 respectively. In this equation the terms $\log (1 - e^{-c_2/\lambda T_1})$, etc., are expanded into a series and only the first term ($-e^{-c_2/\lambda T_1}\log \epsilon$, etc.) is used. As in eq. (2) an approximate value of $c_2 = 14{,}500$ is used in applying the second term correction. For wave-lengths up to 1μ this correction term is small, being only 2.1 and 4.3 for temperatures intervals ($T_2 - T$) of 363° and 623°, respectively, when using $T_1 = 1450°$. However these corrections increase very rapidly with wave-length beyond 1μ so that at 2μ, with the same temperature intervals just mentioned, the second term corrections to the values of c_2 amount to 168 and 227 respectively.

As already stated, it was deemed of greater importance to obtain experimental data than to spend the time discussing the bearing of the data at hand upon existing theories. From the data now at hand, this procedure seems amply justified. More than 180 isothermal energy curves have been obtained and by actual count 75 to 80 per cent. of these curves are found to fit the Planck equation, within the experimental errors of observations. The numerical values of the constants are smaller than the older determinations of Paschen, and of Lummer and Pringsheim (and, for that matter, the earlier values of the present data, obtained by a different system of computation, and not including all the correction factors for reflection). However, as will be shown in the complete paper, the data of previous observers are in agreement with present values, when computed on the same basis.[1]

[1] For example, Paschen s data, if computed by the present methods would give a value of $\lambda_m T = 2{,}912$ and $c_2 = 14{,}460$. The data of Lummer and Pringsheim are wrong owing to an error in their calibration curve which amounts to 0.02 μ for the region of the spectrum up to 2.5 μ. This would reduce many of thei rvalues of λ_{max} by almost 1 per cent., and reduces their mean value to $\lambda_m T = 2{,}930$. Their energy curves did not fit the Wien equation and since in the present research, radiators of their design were used, under conditions which were similar to theirs, it is possible to reduce their data by the present methods of computation.

The data now available were obtained with different fluorite prisms, water cooled shutters, air and vacuum bolometers, and thermocouples. The radiators were platinum-wound porcelain tubes of the design used by Lummer and Pringsheim. Energy curves were obtained from these tubes before and after they were blackened with chromium and cobalt oxides. It was found that the percentage of the energy curves fitting the Planck equation was greatly increased when the radiators were blackened. There are still a few outstanding discrepancies which need further investigation. For example, the average value of c_2 for a given set of observations may be 0.2 to 0.5 per cent. higher than a similar series made, under slightly different conditions, but using the same prism, which was flawless. Usually these fluctuations coincide with variations in humidity, but this is not sufficient to fully explain the matter. Again when using a fluorite prism containing numerous flaws, which caused a conspicuous scattering of light, the energy curves appear to be distorted so that only 25 to 30 per cent. of them fit the Planck equation, and the value of c_2 is extraordinarily high, being of the order of $c_2 = 14{,}700$. The data obtained in 1910 and in 1911 give an average value of $c_2 = 14{,}500$. Using the same prism, in 1912, the value of the constant is slightly lower, being of the order of $c_2 = 14{,}480$. Although this difference of 0.3 to 0.4 per cent. in the values of c_2 is systematic it is within the experimental errors of observation. It therefore appears that the weighted value of this constant, as determined from isothermal curves, will be found to be close to $c_2 = 14{,}500$, and $\lambda_m T = 2{,}920$. The values of c_2, obtained from isochromatics, have not yet been computed.

The constant, c_2, is of great importance in optical pyrometry, and heretofore owing to the absence of concordant data experimenters have used $c_2 = 14{,}500$.

The value of the melting point of platinum, on the basis of the optical temperature scale using $c_2 = 14{,}500$, was observed by Waidner and Burgess to be 1,753° C. The latest work on the gas temperature scale by Day and Sosman gives a value of 1750° to 1755°, with a mean value of 1752°, for the melting point of platinum. A melting point of 1755° is equivalent to a value of $c_2 = 14{,}475$, which is close to the predominating values of this constant as observed in the 9 sets of observations, aggregating 90 spectral energy curves which were obtained in 1912. While it is very gratifying to find that the new value of $c_2 = 14{,}470$ to 14,500 places the melting point of platinum within 1 or 2° from the observed values, this coincidence is to be considered somewhat accidental.

Planck's theory of the mechanism involved in the production of "black" radiation admits of the computation of the value of the elementary electrical charge, e[4] by means of the constant of spectral radiation, $b(\lambda_m T = 2{,}920)$, and the constant, σ, of total radiation. The recent determinations of the constant of total radiation, by Gerlach and by Puccianti give a value of $\sigma = 5.9 \times 10^{-12}$ watts per cm² per deg.[4] Combining this with the value of $b = 0.2920$ cm. deg.

This gives a mean value of $\lambda_m T = 2{,}911$ and $c_2 = 14{,}450$. If we exclude their last value of $\lambda_m T = 2{,}814$, which is evidently not comparable with the rest, the mean value of $\lambda_m T = 2{,}924$ and $c_2 = 14{,}500$. Considered as a whole, a fair estimate of the older data is $c_2 = 14{,}460$ to 14,500.

gives a value of $e = 5.1 \times 10^{-10}$ E.S.U. This is considerably higher than the experimental value by Millikan, which is 4.77×10^{-10} E.S.U. While the time is still premature to attempt to harmonize the experimental value and the theoretical value as deduced from the radiation laws, it is of importance to notice that there are wide variations in the various determinations of the constant, σ, of total radiation. In future communications it is hoped to contribute values of σ, which are determined with some of the apparatus used in determining c_2.

Heretofore, the experimental work on the spectral energy curves was done in the winter time when the humidity was low. The season of low humidity being close at hand, it is purposed to observe several more series of energy curves, using other fluorite prisms (also quartz prisms for the higher temperatures attainable with a vacuum furnace which has been constructed for the purpose), before publishing a more complete report. As matters now stand it seems highly desirable to put the complete optical path of the spectroradiometric apparatus and of the radiator in a vacuum in order to obtain the complete spectral energy curves free from the atmospheric absorption bands of water vapor, carbon dioxide, and oxygen. Having established the form of the energy curve with some degree of accuracy it is purposed to determine with greater exactness the numerical value of c_2 from isochromatics using a fine wire bismuth-silver thermopile, the radiation sensitivity of which is easier to control than is the vacuum bolometer.

WASHINGTON, D. C., December 12, 1912.

AN APPLICATION OF HIGH TENSION OSCILLATORY SYSTEMS TO IGNITION.[1]

BY M. E. GRABER.

THE principal problem in electromagnetic wave propagation is the storing up of a maximum of energy in the aerial with a minimum heat loss in the spark gap.

The inverse problem presents itself in the application of high tension oscillatory systems to gas engine ignition. Here it is necessary to produce a spark of high calorific quality by using smaller capacities and a short spark gap.

In the present investigation of ignition systems, the primary source of current was a Splitdorf magneto with an armature wound to generate from about 7 to 100 volts at from 80 to 700 R.P.M.

After testing out a number of high tension oscillatory systems, a modified Tesla transformer was found to give the best spark for ignition. The diagram of connections for this system is shown in the accompanying figure.

The armature, A, of the magneto supplies the current to the main circuit, which is made and broken by the interrupter, K, once in every rotation of the armature. The oscillatory discharge across the spark gap, PG, serves as an interrupter for the secondary of the Tesla transformer, S.

When C_1 and C_2 are properly adjusted, the secondary, S, responds strongly to the oscillations set up by each primary spark gap discharge and the potential

[1] Abstract of a paper presented at the Cleveland meeting of the Physical Society, December 30, 1912.

difference of its terminals reaches very high values. For the best spark at slow speeds the primary spark gap was .0015″ to .0025″. At higher speeds the primary spark gap was .0025″ to .0035″. The high tension oscillatory discharge seems impracticable for automobile ignition because (1) the hot spark requires adjustments of capacity and spark gaps for the transition from high speed to low speed in order that the two systems may have the same oscillation constant; (2) the secondary condenser insulation breaks down under the high potential stress to which it is subjected; (3) undoubtedly the high calorific quality of the spark at the high speeds was due to arcing, as the hot spark differed materially from the ordinary Tesla spark. The system was tested out on a Cadillac car and an E.M.F. car with satisfactory results for high speeds but indifferent success for slow speeds.

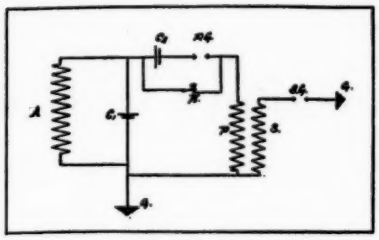


Fig. 1.

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A METHOD FOR CHARGING THE NEEDLE OF A QUADRANT ELECTROMETER.[1]

BY HENRY A. ERIKSON.

TO obtain a conducting quartz fiber of high sensitiveness is difficult. The method here described obviates this difficulty, and has been found very satisfactory.

An insulated ionization chamber A is placed above the quadrants and is separated from them by a metal plate B, which is connected to the case of the instrument, and prevents any direct induction effect from A on the quadrants. The supporting stem of the needle passes through the center of the chamber A, and forms a central electrode. The air inside the chamber A is ionized by means of the rays from a small polonium plate P. The chamber A is connected to a battery, giving the desired potential. The ionization current set up charges the needle to the potential of A.

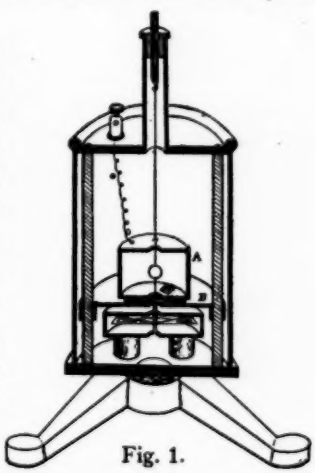

Fig. 1.

Experience thus far has shown that the action of an electrometer with the needle charged by this method is the same as when the needle is charged by means of a conducting fiber.

PHYSICAL LABORATORY,
UNIVERSITY OF MINNESOTA,
December 19, 1912.

[1] Abstract of a paper presented at the Cleveland meeting of the Physical Society, December 30, 1912.

THE DISTRIBUTION OF VELOCITIES AMONG THE PHOTO-ELECTRONS EMITTED FROM THIN CATHODE FILMS WHEN ILLUMINATED BY A MERCURY VAPOR LAMP.[1]

BY PAUL H. DIKE AND F. C. BROWN.

APPARATUS similar to that used by Dike,[2] except the larger dimensions and the more perfect insulation, was constructed with the idea of further identifying the apparent high velocity electrons.

2. The general results, at least as far as the production of high potentials, were verified. Potentials as high as 52 volts were obtained.

3. With a given film the effect decayed by illumination in such a manner as to remove the apparent high velocity electrons first.

4. The apparent high velocity electrons disappeared by resting without illumination, but much slower than by illumination.

5. These apparent high velocity electrons were not obtained by Robinson[3] using apparatus differing apparently only in size and the thickness of the conducting boundary, as well perhaps also in the location and the method of placing the windows.

6. The discrepancy can be explained in one of two ways. Either the reflection of electrons was entirely different in the two cases, or there was in our case something of the nature of an insulating film which gave a condenser effect. In the latter case the quantity of electricity carried over in the deposition of the film would be regarded as bound in a neutral doublet until freed by the ultraviolet light.[4]

7. We tried to detect an insulating film by bringing up a mercury globule, as was done in the previous work referred to, and could detect no such film. This only proves that there was no insulating film formed identical to that previously discovered.

8. Further experiments must be undertaken to settle if the high potentials represent high velocity electrons.

ON THE ORIGIN OF THE EARTH'S MAGNETIC FIELD.[1]

BY L. A. BAUER.

IF attention is confined to the portion of the earth's magnetic field symmetrical about both the axis of rotation and the equator, this portion representing about 90 per cent. of the magnetic energy of the earth's total magnetic field, the following main conclusions have resulted from the author's recent investigations:

1. The portion considered is similar to the field of a uniformly magnetized sphere which has superposed on it another field whose intensity in both hemispheres increases systematically from parallel to parallel, with approach

[1] Abstract of a paper presented at the Evanston meeting of the Physical Society, November 30, 1912.

[2] PHYS. REV., 34, p. 459, 1912.

[3] Phil. Mag., 6, 23, p. 542, 1912.

[4] See paper by Brown, Phil. Mag., 6, 18, p. 668, 1909.

towards the equator. Thus the equivalent intensity of magnetization, in C.G.S. units, is 0.070 for parallels 60° North and South; it then increases systematically toward the equator until it reaches the value of 0.082, which is about 17 per cent. greater than for 60° North and South.

2. Since electricity of both kinds is now known to be an essential and possibly the only constituent of matter, assume that the earth contains two opposite and practically equal charges distributed throughout its interior in such a manner that if the one body is contained within a sphere (or spheroid) bounded by the earth's surface, the opposite body charge is contained within a sphere of slightly smaller radius; in brief, assume that owing to some cause, the volume density of the one charge is slightly different from that of the opposite charge, then the rotation with the earth of the opposite charges produces a magnetic field. Applying this hypothesis, it is found that to harmonize with the most obvious fact of the earth's magnetism—that the north-seeking end of a magnetic needle points below the horizon in the Northern Hemisphere, the volume density of the negative charge must be smaller than that of the positive, or, in other words, the earth's total negative charge must be distributed through the larger sphere and, if that be the whole earth itself, then, for the chief term involved in the magnetic potential, the surface of the sphere containing the positive charge need be, on the average, only 0.4×10^{-8} cm., below that of the earth's surface, *i. e.*, four-tenths of the radius of an ordinary molecule, to give a magnetic field of the required strength. Taking the average atomic weight of the earth's substance in round numbers as 50, the mean volume density of either charge would be about 3.3×10^{12} electrostatic units.

3. Applying the same hypothesis to the portion of the earth's magnetic field which must be referred to systems in the atmosphere, it is again found that the magnetic facts require the negative charge to be distributed through the larger volume. Hence the same kind of electric distribution suffices to produce both the earth's internal and its external magnetic systems. In short, to account for the earth's magnetic field as observed on the surface, it is necessary to assume that the positive and negative electrons in the earth, and in the atmosphere, are distributed in such a way that, on the average, the negative ones (the more mobile ones) are farther away from the earth's center than the positive ones. (The effect resulting from the fact that the observer who measures the magnetic field moves with the electric charges, is very nearly eliminated, owing to the *double and opposite* distribution assumed; the effect, namely, enters with reversed sign for the two opposite charges.)

4. Endowing next the electrons with inertia, then, because of the earth's centrifugal force, there may result possibly a distribution of the electronic systems departing slightly from the simple spherical distribution at first supposed, so that the effective equivalent distribution approaches that of the

[1] Presented before the American Physical Society and Section B of the A. A. A. S., Cleveland, December 30, 1912. The abstract also contains the chief results of the author's paper on "Cosmical Magnetic Fields," presented before the joint session of A and B, December 31, 1912. To be published in Journal Terrestrial Magnetism and Atmospheric Electricity, Vol. 18, 1913.

type represented by the varying surface density of electricity on a conducting oblate ellipsoid of revolution whose minor axis coincides with that of the earth's rotation. Applying this hypothesis, an expression for the earth's magnetic potential is obtained which with three terms, the first, third and fifth zonal harmonics, reproduces very closely the increase towards the equator in the equivalent intensity of magnetization noted in 1 and gives values for the magnetic components, which agree with the observed ones, on the average, to within 0.4 per cent.

5. Some evidences have been found that the theory, or working hypothesis above outlined, will also render valuable aid in accounting for the non-coincidence of the earth's so-called magnetic axis with the axis of rotation, and in the study of the variations to which the earth's magnetic state is continually subject. There is apparently also some indication that we may not have to refer chiefly to the heterogeneity of structure of the earth the fact that its equivalent magnetic poles are not diametrically opposite to each other.

6. The quantity entering finally into the potential of the magnetic field, caused as we have supposed above, is a volume density into a distance which represents the distance between the surfaces of the two spheres (or spheroids), containing the opposite body charges—in other words, an equivalent surface density of about 1.3×10^4 electrostatic units, on the average. Supposing that the latter, as a first approximation, varies directly as the radius, R, of the revolving body, then, so far as the chief term of the potential is involved, the magnetic intensity at the poles is equal to $\kappa\pi\omega R^2$, where ω is the angular velocity of rotation. For the earth $\kappa = 7.0 \times 10^{-16}$ in C.G.S. magnetic units. Applying this constant to the atmosphere and to sun-spots, magnetic fields of the right order of magnitude are found. If the same constant may be applied to the entire sun, the vertical magnetic intensity at the solar poles would be about 300 gausses.

7. There seems little hope at present that a magnetic field caused as supposed above can be detected in the laboratory. Thus for a sphere of 15 cm. radius, rotating 100 times a second, the magnetic intensity at the poles would be but one hundred-millionth part (10^{-8}) of that of the earth and the possible centrifugal effect would only be about one four-billionth part (2.5×10^{-10}).

8. The electrical system which suffices for the production of the magnetic field of the earth and of the atmosphere[1] also appears to be a gravitation system if we suppose that every mass contains a similar system. The force of gravitation, on this hypothesis, is a residual quantity resulting not from a slight difference in the laws of electrical force for the opposite kinds of electricity, as has been assumed, but rather because of non-coincidence of the "centers of action" of the supposed opposite charges, owing to the difference in the volume distribution and the alteration in the distributions by the mutual influence of the attracting bodies upon one another. The distance between the "centers

[1] Professor Hale (Terr. Mag., Vol. 17, p. 178, 1912) has found that if he assumes a similar system for the sun he may account for the polarity of the possible solar magnetic field indicated by his preliminary observations over various zones of the Zeeman effect.

of action" of the attracting bodies is accordingly slightly different for the one kind of electric charge from what it is for the other. The apparent alteration in electrical force for the opposite charges is to be referred, therefore, on this hypothesis to the slight change in the denominator (the square of the distance). The final resultant of all the operating forces may possibly be the force of gravitation; however, this will require further study.

A Comparative Study of the Joule and Wiedemann Magnetostrictive Effects in Nickel Rods.[1]

By S. R. Williams.

A PHOTOGRAPHIC method employed in a former work[2] on these effects in iron was applied to a study of the same effects in two nickel rods. Conditions were found under which nickel behaved as iron for the Joule effect, viz., an initial lengthening for small field strengths and a contraction for strong. Other investigators have found only a decrease in length for all field strengths. Maxwell described the Wiedemann effect as a special case of the Joule effect. One difference seems to have been overlooked by him and others, viz., that in the Wiedemann effect the direction of the resultant magnetization is changing as the longitudinal field is increased, whereas, in the Joule effect it remains unchanged. This change in direction of the resultant magnetic field in the Wiedemann effect is apparently one of the causes why the maximum twist for iron occurs at lower field strengths than does the maximum elongation for iron in the Joule effect. The behavior of these two nickel rods points to a crystalline aggregation of the elementary magnets which mechanical stresses may alter and so affect the two phenomena studied.

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Change in Electrical Conductivity Due to the Orientation of Oblate Spheroids within the Conductor.[1]

By S. R. Williams.

CHANGE in electrical resistance due to a magnetic field was first discovered by Lord Kelvin in 1856. He found that qualitatively a magnetic field increased the electrical resistance when the direction of flow was parallel to the field and decreased it when normal. This effect was found in nickel and iron. Later investigators have confirmed these results for the ferromagnetic substances with which they have worked, but extension of the investigations to diamagnetic substances has shown only an increase of resistance for the various angles between the direction of flow and the magnetic field. Various and somewhat contradictory reasons have been given for this phenomenon and many of the suggested causes must be present as factors to modify the effect.

[1] Abstract of a paper presented at the Cleveland meeting of the Physical Society, December 30, 1912.

[2] Phys. Rev., Vol. 34, p. 258, April, 1912.

In many magnetic phenomena I have found[1] that the different effects may be readily explained by the orientation of ellipsoidal particles within the medium. To make an experiment analogous to that of Lord Kelvin I measured the resistance of an electrolyte containing a group of oblate spheroids. When the equatorial planes were normal to the lines of flow the resistance was increased about 15 per cent. over what it was when the equatorial planes were parallel to the lines of flow. This is in qualitative agreement with the effect discovered by Lord Kelvin who worked with strong magnetic fields, in which the ellipsoidal elementary magnets are supposed to be turned with equatorial planes normal to the magnetic lines of force. Hence the electrical resistance is increased in a direction parallel to the magnetic field and decreased when normal. It may happen that other causes enter to cover up completely the change in resistance due to the orientation of the ellipsoids.

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NOTES ON PROBLEMS INVOLVED IN PRODUCING THREE-COLOR TRANSPARENCIES.[2]

BY CHARLES D. HODGMAN.

THE production of colored transparencies by the three-color process has been the subject of experimental study.

To determine the selection of color-sensitive plates various makes of commercial panchromatic plates have been tested spectroscopically. The construction of filters and their adjustment to the plates used involved similar tests of a large number of dyes and the making and testing of many experimental filters. A simple spectrograph was used, fitted with a plane grating. Photographs of the daylight spectrum were made with a slit wide enough to give a nearly continuous spectrum.

The positives were made by the well-known method of Du Hauron, used later by Mr. Ives. Various dyes were used for staining and the effects observed of variation in the concentration of the dye solutions and the time of staining. The best results were obtained with solutions of greater concentration and shorter time of immersion than are usually employed.

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[1] PHYS. REV., Vol. 34, p. 40, January, 1912.

[2] Abstract of a paper presented at the Cleveland meeting of the Physical Society, December 30, 1912.